THURSDAY, AUGUST 5, 1875

AMERICAN GEOLOGICAL SURVEYS

THE United States of America have certainly done noble work in the exploration and mapping of their vast empire. Most of the long-settled States have for many years possessed elaborate maps and reports upon the topography, geology, and agricultural features of their territory. The central Government has likewise carried on extensive and admirable coast surveys, besides innumerable expeditions and surveys for opening up the less known or wholly unvisited regions of the interior of the continent. Were all the literature connected with this subject gathered together it would be found to form of itself a goodly library. Some of it has been published in most costly and indeed luxurious style; other portions, and these sometimes not the least interesting or valuable, have to be unearthed from the pages of flimsily printed "blue-books." But whatever be their external guise, these narratives are pervaded by an earnestness and enthusiasm, a consciousness of the magnitude of the scale on which the phenomena have been produced, and yet a restrained style of quiet description, which cannot but strike the reader. Their writers have evidently had their feelings of awe and admiration worked sometimes up to the highest pitch, yet they contrive on the whole to present just such plain frank statements of facts as to convey clear and definite notions of the regions they describe. Though little is said about hardships and hair-breadth escapes, one can see that these bold explorers could not have accomplished what they so modestly and quietly narrate without a vast amount of privation and danger. Some of them, indeed, like poor young Loring in 1871, have lost their lives by Indian assassins, others have fallen victims to the disease and debility necessarily attendant on so much exposure. But on the whole the work seems to be healthy, and the men engaged upon it like it and keep to it.

Leaving for the present the consideration of what has been done and is now doing in the more settled States, let us turn to those vast territories lying to the west and stretching across the Rocky Mountains to the shores of the Pacific. At the beginning of this century comparatively little was known of these regions. But the Government then resolved to gather some information on the subject, and with that end despatched an expedition in 1804 which penetrated the wilderness, reached the western sea-board, and after much hardship brought back a first instalment of knowledge regarding this part of the continent. During the period preceding the year 1851, somewhere about forty exploring and survey parties were sent by the War Department into the tracts lying to the west of the Mississippi. But in the next twenty years, viz., from 1850 to 1870, the same Department conducted forty-six of these surveys, not merely for military purposes, but to aid in the general opening up of the vast unexplored territories. As a rule, however, and until comparatively recently, these expeditions could make no pretensions to geographical accuracy. Their object was merely to fix as well and as rapidly as might be the positions of main landmarks, and to collect such information as to the nature of the country as was most needful, with the view to its early settlement.

But the discovery of gold in California at once drew attention to the western slope, and awakened a strong desire to open up a better and more expeditious communication with it than had previously been in use. The Pacific Railroad was projected, and surveys were made to ascertain the best routes. In the course of these explorations much additional information was obtained, but still in such necessarily rapid work there could be little done towards accurate geographical and topographical determinations. Hence we find that prominent points were sometimes placed from three to twenty miles out of their true position. Nor could much be attempted of any value in a geological point of view. It is seldom that a single traverse of the rocks of a wide region can be understood without a knowledge of the country lying on either side of it.

In a region of which no reliable maps exist, it is of course impossible to conduct a geological survey except in connection with a topographical one. The geologists must either make their own topographical maps, or be accompanied by surveyors who do that for them. Previous to the year 1867 no special geological exploration seems to have been carried on in the territories as the work of any Government Department. But in that year no fewer than three separate and independent geological surveys were organised. One of these, under the direction of the War Department, but conducted entirely by civilians, with Mr. Clarence King at their head, made a careful examination of a tract about a hundred miles broad, stretching along the fortieth parallel, from the eastern boundary of California to the eastern slope of the Rocky Mountains. A second survey, under the direction of the Smithsonian Institution, with Mr. J W. Powell in charge, had as its task the exploration of the Colorado of the West and its tributaries. A third survey, or series of surveys, has been conducted with great zeal by the Department of the Interior over a vast range of country embracing Nebraska, parts of Colorado and New Mexico, Wyoming, Utah, Montana, and Idaho. These surveys have been under the guidance of Dr. F. V. Hayden.

There appears to have been no concert between the different Government Departments in the organisation and conduct of these various geological explorations. Each survey party was sent out as if it had the boundless wilderness to subdue without the aid of any compatriots or even the chance of seeing human beings save hostile Indians. The Territories, though vast, were not infinite, and it was to be expected that some time or other the independent survey parties should meet. This does not seem to have happened for some years. Meanwhile, however, Dr. Hayden's expedition, supported by increasingly liberal grants from Congress, was doing most excellent work, making a good general map, and at the same time bringing before the world an annual report full of most interesting and valuable and sometimes remarkably novel information regarding the geology and natural history of the regions visited. The War Department, with a far more powerful organisation, and with the help of a staff of trained civilians, was much more deliberate in its movements. Very little of its work had seen the light, though of the excellence and copiousness of that work

there was no reason to doubt. As the Department had been for more than half a century in undisputed command of the exploratory expeditions of those western regions, perhaps some of its more zealous functionaries may have grown somewhat jealous of the increasing popularity of the work done by the Department of the Interior, and may have looked upon that work as an unwarrantable encroachment upon the recognised province of the military corps. Be this as it may, a chance meeting of two independent survey parties in 1873, and the fact that to a certain extent they both surveyed the same ground, led to a battle royal in the spring of last year, wherein appeared the chiefs of the Departments with President Grant at their head, military men, geologists, naturalists, topographers, and several cohorts of professors. Evidently some of the parties knew that the contest would come sooner or later, and were prepared accordingly. The first bomb-shell was thrown as it were by an outsider, on the 15th of April, 1874, when Mr. Lazarus D. Shoemaker carried a resolution in Congress requesting the President to inform the House what geographical and geological surveys were carried on by the Government in the same or contiguous areas of territory lying to the west of the Mississippi, and whether these could not be combined under one Department, or at least have their respective geographical limits defined.

The question thus raised turn out to be really whether the War Department should have entire control of the surveys, both those intended for military and those for purely civil purposes. The President replied that they would be more economically and quite as efficiently carried on by the military authorities. Not content with this recommendation of its military chief, Congress referred the matter to its Committee on Public Lands. A careful investigation followed, and though the military side fought hard for its supremacy, the Committee decided against the purposed consolidation. Their conclusions ran thus: "That the Surveys under the War Department, so far as the same are necessary for military purposes, should be continued; that all other Surveys for geographical, geological, topographical, and scientific purposes should be continued under the Department of the Interior, and that suitable appropriations should be made by Congress to accomplish these results."

There can be little doubt that though it must have chagrined some sanguine partisans whose ebullitions of temper form an amusing feature in the congressional blue-book, this decision of the Committee was in the circumstances a wise one, and one which, followed out by the Government, will have an important influence in the development of the vast and still unexplored regions over which the surveys have yet to extend. It is impossible that the corps of Engineers, weighted with all the numerous and arduous duties which form its ordinary work, should be able to furnish the necessary complement of trained geologists, botanists, naturalists, and other scientific men for the adequate exploration of the territories. In fact, the scientific work of that corps has all along been done in great measure by civilians. But it is neither needful nor desirable that civilians of high training in practical scientific work should be placed under military direction. They move more freely without it. And as in the Western Territories they declare that they no longer need the protection of an escort, the sole remaining reason for a military supervision would seem to be removed.

The Surveys of the Department of the Interior claim the first place from their voluminousness and from the wide area to which they refer. As already mentioned, they have been carried on since their beginning by Dr. F. V. Hayden, to whose skill in geological work, and tact in diplomatic relations with Government bureaux, officials, and fellow-labourers in science, their success is certainly in large measure due. For the last twenty-two years he has given himself to the exploration of the north-western territories. In the spring of 1853 he ascended the Missouri in one of the American Fur Company's steamboats and spent three years up there, during which time he accumulated considerable collections in natural history. In 1856 he joined an expedition of the Engineer Topographical Corps to that region as surgeon and naturalist. On the outbreak of the Civil War he took service in the Federal Army, as Surgeon of Volunteers, and served four years. But when the war ended, finding himself out of employment, he in 1866 returned to the north-west on his own resources, and resumed his researches in the natural history of that region. In the following year, Congress having made a small grant of $5,000 towards a Geological Survey of Nebraska, Dr. Hayden received the charge of it. This was the beginning of his career as Government geologist. But his path was not strewn with roses, either amid the hills of Nebraska or in the Government Offices at Washington. The sum appropriated for his survey in 1867 was the unexpended balance of the grant for the legislative expenses of the territory. He had a sore fight to get it renewed next year. But in 1869 Congress took up the question in a broader spirit, and sanctioned a general geological survey of the Territories of the United States, with an appropriation of $10,000 to be administered by the Department of the Interior. Since that date, owing no doubt to the marked success of the Survey, the gran has grown rapidly in amount, till at present it stands at $75,000.

This great increase in the amount of funds at his disposal has enabled Dr. Hayden to augment and equip his staff to an extent very different from that of his modest beginning in 1867. According to his last published report he organises his force into three geological parties, each completely furnished and able to act independently, so that if desired it could be transferred to any portion of the public domain. Each of these parties consists of a topographer, an assistant topographer, a geologist, two packers, a cook, and usually two or three others as general assistants or collectors in natural history. But besides these he has still three other parties, one for the purpose of carrying on the primary triangulation of the country and thus correcting and harmonising the trigonometrical work of the other or geological explorers, a second for procuring photographs and information likely to be useful to the other parties and the public, and a third, and not least important, the quartermaster's party, for furnishing supplies to all the others. These three last-named parties traverse the entire field of work.

A mere inspection of the catalogue of the publications of the Geological Survey of the Territories is enough to

show what an enormous amount of work has been got through in seven years. First of all there is an Annual Report of Progress, in which, without waiting for completed surveys, the general results of each year's work are given, in geology, palæontology, mineralogy, natural history, meteorology, archæology, and economic products of every kind. Then come what are called Miscellaneous Publications and "Bulletins"—little pamphlets giving data in meteorology, topography, natural history, or other information gathered in the course of the Survey. Next we have large quarto monographs, admirably printed and illustrated, devoted to the discussion of the more technical and matured results, such, for instance, as the palæontology of a wide region or of a formation. Lastly, a series of topographical maps of parts of the districts surveyed has been published. These will be of great value as a basis for the general map to be afterwards constructed. Geologists in this country accustomed to the elaborate geological maps issued by our Government, may perhaps at first wonder why geological maps, properly so called, do not appear among the publications of the Geological Survey of the Territories. But the delay in the issuing of a general map is as necessary as it is prudent. A report may be written of what one sees. It is complete in itself; and if it is found to contain errors, these can be corrected in a subsequent report. But a sheet of a geological map must fit accurately to its neighbours. If it is surveyed and published without waiting for the investigation of the surrounding area, it will most probably be found somewhere, at least, erroneous; and to make it harmonise with adjoining sheets may require so much alteration as to demand, perhaps, even the cancelling of the old and the engraving of a new plate. Therefore we are content to wait for Dr. Hayden's geological map of the Territories in confident anticipation that it will be worthy of the high reputation which he and his staff have already gained.

It should be added, that with the most praiseworthy liberality the publications of the Survey are distributed as gifts to learned bodies and scientific men all over the world. All that is asked is that, where possible, the scientific publications of the recipients of the volumes may be sent in exchange. It is to be hoped that this generous spirit has called forth a similar feeling elsewhere, and that the library of the Geological Survey of the Territories is continually augmented by presents from all parts of the world.

ARCH. GEIKIE

FISKE'S "COSMIC PHILOSOPHY"

Outlines of Cosmic Philosophy, based on the Doctrine of Evolution, with Criticisms on the Positive Philosophy. By John Fiske, M.A., LL.B., Assistant Librarian, and formerly Lecturer on Philosophy, at Harvard University. 2 vols. (London: Macmillan and Co., 1874.)

WE have repeatedly expressed our admiration of the system of philosophy which Mr. Spencer is engaged in working out. Mr. Fiske, in giving an outline of this philosophy, has called it *Cosmic;* a name which he thinks peculiarly fitting, because "the term 'Cosmos' *connotes* the orderly succession of phenomena quite as forcibly as it *denotes* the totality of phenomena; and with anything absolute or ontological, with anything save the 'Mundus' or orderly world of phenomena, it has nothing whatever to do." But Mr. Spencer is far from ignoring the absolute, and the ontological element in his speculations has frequently been the subject of criticism; and surely Mr. Fiske goes beyond an account of the orderly succession of phenomena in all that he has to say about the "Infinite Power manifested in the world of phenomena," which he finds that we are clearly bound to symbolise as quasi-psychical rather than as quasi-material, so that we may say with meaning, "God is Spirit, though we may not say, in the materialistic sense, that God is Force."

As the Evolution-Philosophy, which is for the most part but higher science, has swallowed up the rival systems of former times, and now stands itself without a rival, we need not pause to speak of its merits. Our first duty then is to acknowledge that Mr. Fiske has succeeded in giving a very faithful and attractive sketch of Mr. Spencer's philosophy. He has made all the thoughts his own, and has, we should think, secured for himself a recognised place among the most advanced thinkers of our time. But Mr. Fiske claims that his work shall be regarded as more than a mere reproduction of Mr. Spencer's thoughts. It contains "much new matter, both critical and constructive." In relation to the evolution of society, the author supposes he has anticipated what "will doubtless be much more thoroughly and satisfactorily presented by Mr. Spencer in his forthcoming work on Sociology." Without stopping to inquire whether a love of system may not here, as elsewhere, have led to a slight waste of energy and a straining of words, it must without doubt be recognised that Mr. Fiske has expressed with clearness and ability many large and important truths, the recognition of which must have a very healthy and elevating effect. Nothing can be better than for people to reflect that moral progress consists in the continual "adaptation of the desires of each individual to the requirements arising from the coexistent desires of all neighbouring individuals." Again, the superiority of a true philosophy over some modes of thought which still claim to be the most advanced, may be learned from Mr. Fiske's profound appreciation of the vital part played by the Roman Church in the evolution of European civilisation.

The original matter, however, on which the author lays most stress, refers to the genesis of man. He works out a theory as to the part taken by the prolongation of human infancy in originating social evolution, which, in his own words, "is entirely new in all its features." To account for the passage from mere gregariousness to sociality as marked by permanent family groups, is the problem Mr. Fiske has set himself, and his solution is this:—Mr. Wallace has given a most beautiful exposition of the operation of natural selection at that point in the evolution of man from a lower form when variations in intelligence began to be seized on and preserved rather than variations in bodily structure. It was then that our remote progenitors began to clothe their bodies and to prepare their food, that the ape of many devices survived where his perhaps stronger or swifter contemporaries perished. Now, increase in intelligence, says Mr. Fiske, implies increase in size and complexity of brain; and, as a matter of observation, this structure, as it becomes more and more complex, is less and less definitely organised at

birth; then arises the phenomenon of infancy. The orang-outang, until about a month old, "lies on its back, tossing about and examining its hands and feet;" with the lowest savages the period of helplessness is much longer, and as civilisation advances, the period during which the child must depend on the parent for support, becomes still longer. Mr. Fiske believes that these considerations supply "a very thorough and satisfactory explanation of the change from gregariousness to sociality." "The prolonged helplessness of the offspring must keep the parents together for longer and longer periods in successive epochs; and when at last the association is so long kept up that the older children are growing mature, while the younger ones still need protection, the family relations begin to become permanent. The parents have lived so long in company that to seek new companionships involves some disturbance of ingrained habits; and meanwhile the older sons are more likely to continue their original association with each other than to establish associations with strangers, since they have common objects to achieve, and common enmities, bequeathed and acquired, with neighbouring families. As the parent dies, the headship of the family thus established devolves upon the oldest, or bravest, or most sagacious male remaining. Thus the little group gradually becomes a clan, the members of which are united by ties considerably stronger than those which ally them to members of adjacent clans, with whom they may indeed combine to resist the aggressions of yet further outlying clans, or of formidable beasts, but towards whom their feelings are usually those of hostile rivalry." "In this new suggestion," says Mr. Fiske, "as to the causes and the effects of the prolonged infancy of man, I believe we have a suggestion as fruitful as the one which we owe to Mr. Wallace," and "the clue to the solution of the entire problem" of the origin of the human race.

Towards the end of the second volume there is a good deal of more or less original matter relating to religion, much of which we think open to serious criticism, but on which we cannot enter here. There is, however, in the writings of Mr. Spencer, a view (adopted by Mr. Fiske) with respect to the relation of feeling to movement which appears to us to be nothing more than a popular fallacy, and which, as it seems to us, spreads much confusion through the psychological part of his system. The assumption against which we would direct some criticism is, that feelings stand in a causal relation to bodily movements. The point has recently occupied some attention, but we must reserve our remarks for another article.

Though we admire the far-reaching speculations of Mr. Spencer as more wonderfully consistent than the thoughts of any other thinker of equal range, we cannot regard his writings as criticism-proof at all points. Mr. Fiske, in arguing against the volitional theory of causation, says: "Phenomenally we know of will only as the cause of certain limited and very peculiar kinds of activity displayed by the nerves and muscles of the higher animals. And to argue from this that all other kinds of activity are equally caused by will . . . is as monstrous a stretch of assumption as can well be imagined." "Because this is the only cause of which we are conscious, . . . we are asked to assume, without further evidence, that throughout the infinitely multitudinous and heterogeneous phenomena of nature no other kind of cause exists. A more amazing example of the audacity of the subjective method could hardly be found." We hope soon to see the evolution philosophy rendered at once more consistent with itself, and able to give to the volitionist a more complete answer than is to be found in this "crushing refutation;" at which the volitionist will but smile, believing the strong language to be but a make-weight to the weak argument. The argument, as it stands, is Mr. Fiske's; it is in the admission made to the volitionist, viz. that certain movements are caused by feeling, that he follows Mr. Spencer. We contrariwise maintain that an antecedent feeling is never the cause of any movement whatever, that there is no evidence of its being so, that the phenomena of life and motion can be wholly accounted for without such assumption; that the assumption, that feeling causes movements, though it can be expressed in words, cannot be represented in thought; and that the thing asserted is inconsistent with the physical explanation of the objective side of the universe—of all physical phenomena, and movements are such—which is a fundamental idea in Mr. Spencer's philosophy. When this is accepted, the answer to the volitionist will be, that he takes for the cause of all action not that which is phenomenally known "only as the cause of certain limited and very peculiar kinds of activity," but that which is not known to be, and cannot be conceived of as, the cause of any activity.

Justice cannot be done to this criticism in a review article in these columns. We shall therefore content ourselves with calling attention to some of the confusion which, as it seems to us, this popular fallacy introduces into the philosophy Mr. Fiske expounds in these volumes. First, let us have no misunderstanding, if that be possible among philosophers. Certain states of consciousness, which precede certain bodily movements, and which are called by the learned "volitions," have in all ages been believed to be the cause of these movements. This opinion is perhaps as ancient as the human mind, more ancient than, and the father of, the earliest conceptions of deity. It is still the all but universal opinion, not of the vulgar, but of the most cultured. Quoting from Mr. J. S. Mill, Mr. Fiske says: "Our will causes our bodily actions in the same sense (and in no other) in which cold causes ice or a spark causes an explosion of gunpowder." In a passage quoted from Sir William Hamilton, also with approval, we have this definite expression: "A multitude of solid and fluid parts must be set in motion by the will."

Now let us in effect deny all this in Mr. Fiske's own words. Speaking of what he calls "the closed circuit of motion, motion, motion," he says: "No conceivable advance in physical discovery can get us out of this closed circuit, and into this circuit psychical phenomena do not enter. Psychical phenomena stand *outside* this circuit, *parallel* with that brief segment of it which is made up of molecular motions in nerve-tissue." "However strict the parallelism may be, within the limits of our experience, between the phenomena of mind and this segment of the circuit of motions, the task of transcending or abolishing the radical antithesis between the phenomena of mind and the phenomena of motions of matter must always remain an impracticable task. For in order to transcend or abolish this radical antithesis we must be

prepared to show how a given quantity of molecular motion in nerve-tissue can become transformed into a definable amount of ideation or feeling." Strange that it does not occur to our philosophers that they just leap this impassable gulf from the other side when they talk about a multitude of solid and fluid parts being set in motion by the will, in the same sense in which a spark causes an explosion of gunpowder. Either the volition is itself a mode of motion, which Mr. Fiske solemnly denies, or the circuit is not closed, which he as solemnly asserts it to be.

The inconsistency and consequent error, to which we have called attention, cause much more widespread confusion than might at first be supposed. In one direction we have seen the closed circuit of motion broken in on. In the opposite direction we have elaborate attempts to evolve mind out of matter, all specific and impressive declarations to the contrary notwithstanding. In this direction Mr. Lewes has gone forward with a more uncompromising logic than is to be found in the volumes before us. Mr. Fiske agrees with Mr. Lewes that both "life and mind are processes," but we do not find that he goes on to picture consciousness "as a mass of stationary waves formed out of the individual waves of neural tremors." The evolution philosophy, starting from the primeval nebula, finds every science a specialised part of some more general science. Biology is a specialised part of geology, and psychology is a specialised part of biology. "Mind here appears," says Mr. Fiske, "to be but the highest form of Life," and life, as admirably defined by Mr. Spencer, "is the definite combination of heterogeneous changes, both simultaneous and successive, in correspondence with external co-existences and sequences." Truly the study of the higher forms of these phenomena may be called a specialised part of biology. But may we call any adjustment of internal relations to external relations *Mind?* We think not, and in this Mr. Fiske heartily agrees with us, for he hastens to tell us that "push our researches in biology as far as we may, the most we can ever ascertain is that certain nerve-changes succeed certain other nerve-changes or certain external stimuli in a certain definite order. But all this of itself can render no account of the simplest phenomenon of consciousness." And Mr. Spencer is equally emphatic:—"The thoughts and feeling which constitute a consciousness form an existence that has no place among the existences with which the rest of the sciences deal." But where are we now? If in psychology any part of the phenomena studied are those given directly in consciousness, then they are not the phenomena which form a specialised part of biology. Consciousness, then, is not evolved out of the primeval nebula. It creeps in surreptiously somewhere in the course of the evolution of organised beings, and appears in man, the highest product of evolution, as a power guiding his movements. This, to our mind, is the weak point in Mr. Spencer's philosophy.

Let us glance at Mr. Fiske's chapter on the Evolution of Mind, which he tells us "was mostly written, and the theory contained therein entirely worked out, before the publication of Part V. of the second edition of Mr. Spencer's 'Principles of Psychology.'" In so far as this so-called theory of the evolution of mind is an account of the evolution of the nervous system, it may be open to no serious criticism. But what happens is this: From talking of waves of molecular disturbance passing along finished channels and finding for themselves new courses in lines of least resistance, the language gradually changes; a process entirely physical, "reflex action, which is unaccompanied by consciousness," is called "the simplest form of psychical life." Instinct is found to be compound reflex action. And in the higher organisms "there will be a number of permanent transit-lines and a number of such lines in process of formation, along with a continual tendency towards the establishment of new ones. The consequences of this are obvious. In becoming more and more complex, the correspondence becomes less and less instantaneous and decided. 'They gradually lose their distinctly automatic character, and that which we call instinct merges into something higher.'" What is the something higher into which all these nervous operations merge? Into mind as we see it in man, who is supposed to perform actions "with the assistance of reason, volition, and conscious memory."

It is, however, when specially engaged with the consideration of voluntary action that the confusion may be said to reach a climax. But Mr. Fiske has no misgiving; he proceeds, confident that he has clear ideas to expound, and that he is expounding them in clear and consistent language. "Volition," he tells us, "is that transformation of feeling into action which is attended by a conscious comparison of impressions." If *feeling* may be transformed into *action*, why may not motion be transformed into feeling? Having written this he cannot well afford to sneer at the materialist. Though mind and motion, as we are often told, have no kinship, yet here are a few sentences which are perhaps expected to help us towards a mental picture of the curious "dynamic process" "whereby feeling initiates action."—"In a complex aggregate, like the human or animal organism, such a state of equilibrium (as the ass between the two bundles of hay) cannot be of long continuance. Sooner or later—either from the greater vividness with which one of the desired objects is mentally realised, or from any one of a thousand other disturbing circumstances down to those of a purely physical nature—one desire will become stronger than the other, and instantly thereupon, the surplus nervous tension remaining after the weaker desire is neutralised, will pass into nervous *vis viva*, or, in other words, volition will take place." It will be almost a sufficient criticism of these statements to place alongside of them a sentence from Mr. Fiske's next paragraph. "To say explicitly that volition does not follow the strongest motive, is to say implicitly that motion does not always follow the line of least resistance; which is to deny the persistence of force." With this last statement we agree; but how is it to be reconciled with the preceding sentences? Can mental vividness, or anything else not purely physical, either help or hinder motion in following the line of least resistance? To say so is to deny the persistence of force.

Having found that philosophers are very like other people, that they are sometimes almost as anxious to be thought infallible as to have any inconsistency in their writings pointed out (Mr. J. S. Mill was a grand exception), it may perhaps be as well to say that in bringing together a few passages which seem to us after careful

consideration to be hopelessly inconsistent, we have been inspired by no other feeling than a desire to see the philosophy we admire purified from an error that greatly mars its beauty. Let it be accepted that states of consciousness really stand outside the circuit of motions and therefore can never be a cause of any movement, and the occasion of all the confusion of which we have spoken disappears.

Mr. Spencer, who has been so kind as to read the proof of this article, tells me by letter that he thinks I have not quite remembered his point of view and its implications. He says:—"The implication of your argument seems to be that I identify motion as it actually exists with motion as manifested to our consciousness. Did I do this there would be the inconsistency you allege in the supposition that feeling is transformable into motion and motion into feeling. . . . But that transformation which I assume to take place (though without in the least understanding how) is the transformation of the subjective activity we call feeling (unknowable in its ultimate nature) into the objective activity we call motion (also unknowable in its ultimate nature)." On the metaphysical question my own view probably does not differ much from Mr. Spencer's; but I would have it kept distinct from the question of ordinary science, which deals only with the relations of things as manifested to our consciousness. And I leave it to Mr. Fiske and his readers to determine whether in the passages I have quoted from his work he means motion and feeling as known to us—the motion and feeling of science, or the ontological entities of the metaphysician, with which in his preface he has told us his system "has nothing whatever to do."

DOUGLAS A. SPALDING

OUR BOOK SHELF

Notes on the Fertilisation of the Cereals. By A. S. Wilson. (Reprinted from the "Transactions of the Botanical Society of Edinburgh.")

NOTWITHSTANDING the practical importance to the farmer of a knowledge of the mode in which our cereal crops are fertilised, it is singular that different views still prevail on several essential particulars. One point appears to be generally conceded, that insects have nothing to do with it; the ovules are either self-fertilised, or cross-fertilised by the agency of the wind. Dr. Boswell-Syme and the present author incline towards the former; Delpino and Hildebrand to the latter view, at all events in the case of wheat; and Belgian farmers still trail ropes over their flowering wheat to insure complete fertilisation. Although we cannot altogether agree with Mr. Wilson's conclusions, he has added some most valuable observations to our knowledge of the subject, especially with regard to the remarkable extension of the filaments immediately previous to, or concurrently with, the discharge of the pollen. If a rye-flower, he states, is opened a moment before the natural time of flowering, the filaments will be found to measure about one-sixteenth of an inch in length. In the course of five minutes, or less, from the instant the pales begin to open, the filaments will, in many cases, have grown to twelve-sixteenths, and the whole of the pollen will have fallen out; and this rapid extension is not a mere straightening out of a doubled-up thread, but an actual growth. In oats and barley a similar extension takes place; in the latter case the filaments may be seen, under an ordinary pocket lens, to be visibly growing at the rate of six inches an hour. The result at which Mr. Wilson arrives is, that the "European cereals are self-fertilised, and that the act of fertilisation, in those cases in which the flower opens, is probably performed in the opening, and is necessarily confined to the twenty or thirty minutes during which the flower remains open." We must confess that we are not convinced of the validity of the train of reasoning which led the author to this conclusion. The remarkable phenomenon of the extension of the filaments would appear to be quite useless for this purpose, Mr. Wilson's drawing showing that its effect is to remove the anthers from the immediate proximity of the stigmas to a considerable distance from them. The whole mechanism of the "versatile" anthers, lightly suspended at the end of very slender filaments, the immense quantity of light dry pollen, and the sudden jerk by which the flowers are opened, appear to lead *primâ facie* to an opposite conclusion, and to suggest the agency of the wind. On two other points Mr. Wilson seems to us to have been led into some confusion by an incorrect use of terms. He speaks of the meaning of the word "fertilisation" as being "partly a matter of convention; it may mean that act of the anthers by which they project or discharge the pollen, which, falling directly on the pistil, shall produce the embryo; or it may mean the falling of the pollen on the ovule after being carried a distance by the wind; or it may apply to the instant in which the elements of the pollen set up that action in the ovule which produces a new plant;" and he employs the word throughout in the first of these meanings. Now we believe that all our best writers use the term as synonymous with "impregnation" or "fecundation;" and that the correct expression for the falling of the pollen on the stigma—the German "Bestäubung"—is "pollination"; Mr. Wilson's "fertilisation" being simply the discharge of the pollen from the anther, which may or may not "pollinate" the stigma and "fertilise" the ovules. He also finds fault with those botanists who distinguish between "cross-fertilisation" and "self-fertilisation"—the fertilisation of ovules by pollen from a different or from the same flower—without being able to define accurately the physiological difference between the two processes. The terms are, however, currently used, and we think quite correctly, to express an actual external difference, which we know from experience to be frequently accompanied by results of a different character; even though we are not at present able to trace this difference to its physiological causes. Notwithstanding these points, to which we have felt bound to call attention, the present treatise is one of the most important contributions yet made to our knowledge of the remarkable phenomena connected with fertilisation.

A. W. B.

Official Guide to the Kew Museums; a Handbook to the Museums of Economic Botany of the Royal Gardens, Kew. By Daniel Oliver, F.R.S. Sixth edition, with additions by J. R. Jackson, A.L.S., Curator of the Museums. (J. R. Jackson, Museum, Kew. 8vo., 92 pages.)

ALTHOUGH this is by no means a complete catalogue or guide to all the objects exhibited in the museums at Kew, very few substances of commercial importance have been overlooked. Necessarily in so small a book, little is said of the relative value, &c., of different fibres and other vegetable substances; but it will be found useful to all interested in applied botany, inasmuch as it embodies all recent discoveries of interest to the druggist, manufacturer, or artist. The products are arranged in families according to their affinities, and by means of this guide, which has a complete index of trivial and technical names, the visitor can readily find any article of which he may be in search. One thing, however, is certain, the Government might, by a small grant in aid of a more comprehensive publication, render the fine collection of vegetable products at Kew of infinitely more service to the general public.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

On the Mechanical Work done in exhausting a Muscle

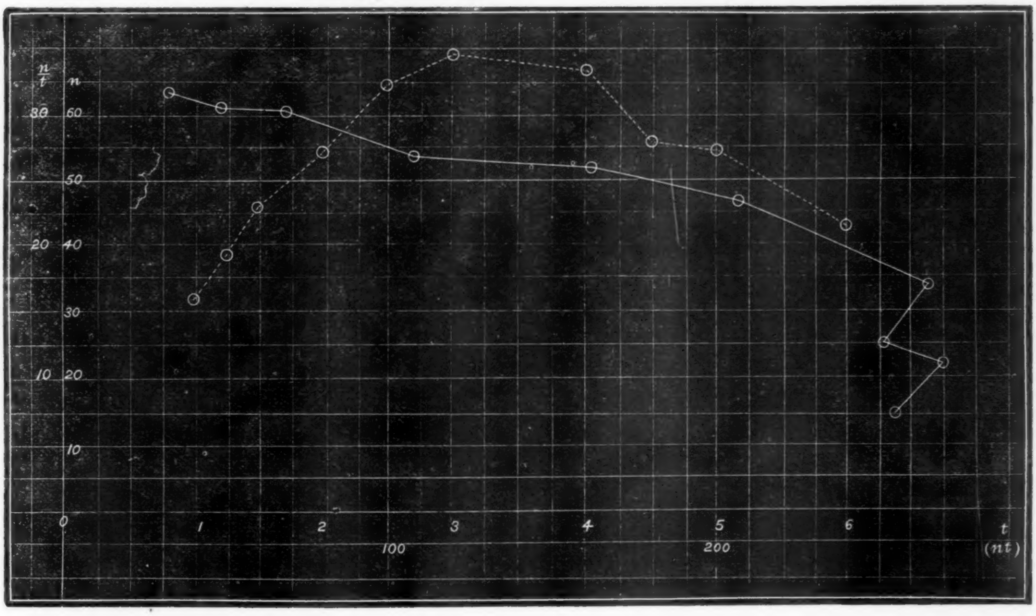
I BEG leave to make some reply to the comments (NATURE, vol. xi. pp. 464, 488) of Prof. Haughton on my paper.

1. In regard to the relative value of my earlier and later experiments, it is to be said, that in one sense they are all equally valuable. My object, however, was to find the work of exhaustion when the intervals of work and rest were equal, the work to be expended only in lifting the weight. Hence the experiments were made in such a way as to eliminate the fatigue caused by the falling weight. Prof. Preston and myself practised for several weeks, until we were able to keep accurate time, before the published series was begun. All who saw the experiments were then satisfied that the later method of experiment was an improvement. The two series first published were made with equal care, and I am therefore at some loss to know what has been Prof. Haughton's criterion in deciding that one was good and the other bad.

2. In dealing with Prof. Haughton's equation—

$$n = \frac{A.t}{1 + \beta t^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

when it was said that the co-ordinated values of $\frac{n}{t}$ and $n't$ formed a curve, the meaning could only have been that β is not a constant. Prof. Haughton is of course right in saying that the observations thus co-ordinated "may be represented by a straight line." He might also have added that for properly chosen limits, any other observations may also be represented by a straight line. The point is, whether these lines give any evidence of regular deviations. It seems to me that "any one accustomed to such observations" ought to be able to see such evidence in the diagrams of Prof. Haughton in NATURE, *loc. cit.* In this connection I wish to give a series of experiments, the time of lift t being variable and equal to the interval of rest. The values of n are the means of four experiments, and are uncorrected for variations in strength. The experiments were made with the apparatus described in my last paper (NATURE, vol. xi. pp. 256, 276) and my right arm.



Raising weight of 4·5 *kgr. through* 0·70 *in* t *sec.*

t	n	$\frac{n}{t}$	$n't$
1·00	31·7	31·7	31·7
1·25	38·7	30·6	48·4
1·50	45·5	30·3	68·3
2·00	54·0	27·0	108·0
2·50	64·7	25·9	161·8
3·00	69·2	23·1	207·6
4·00	66·5	16 6	266·0
4·50	56·0	12·4	252·0
5·00	54·0	10·8	270·0
6·00	42·5	7·1	255·0

The values of n and t are represented in the diagram by the dotted line. It will be seen that n reaches a maximum where $t = 3·4$.

The values of $\left(\frac{n}{t}\right)$ and $(n't)$ are also represented by the full line. It will be observed that the observations on opposite sides of the maximum n are not continuous. A comparison of this line with those given by Prof. Haughton in NATURE, vol. xi. p. 465, will be found instructive.

3. In the case just considered, the time of exhaustion depends upon the velocity of disintegration and recuperation of the muscles. It is well known that the velocities of such operations, taking place in time, are represented by the binomial curve. I have satisfied myself that the values of n in the above series are represented by the terms of the expanded binomial $(p + q)^m$ where $p + q = 1$; where p and q are unequal, and where $m = 1$ represents the total number of chances. This point is reserved for future investigation.

4. In my paper in the *American Journal of Science* (Feb. 1875, pp. 130–137), the accuracy with which Prof. Haughton's formula represents my experiments, was shown. Assuming

$$(w + a)\, hn = \frac{A}{(w + a)^v}$$

where a is the reduced weight of the arm, and Prof. Haughton's law demands that v = unity. It turns out to be 2·6. Prof.

Haughton refers the difficulty to my experiments, and I refer the difficulty to his theory.

5. Prof. Haughton objects to my reduction for variations in strength. In reply, it is to be said that an increase of from 13·66 kgr. to 14·84 kgr. in the strength of Mr. Myer's arm, caused *n* to vary from 78 to 1366. The weight used was 5·00 kgr. For a weight of one or two kgr. my own arm also varies thus greatly. I therefore conclude that this reduction is not only not improper, but that it is essential.

6. I beg leave to state that I alone am responsible for the paper published in NATURE, vol. xi, pp. 256-276. I acknowledged therein all the aid that I am conscious of having received. F. E. NIPHER

Washington University, St. Louis

P.S.—I find that one important point in Prof. Haughton's paper has been overlooked in my reply. Objection is made to my last series of experiments, on the ground that all the muscles thrown into action are not exhausted. If this objection is well taken, it applies also to the former series of mine, so "highly confirmatory of the Law of Fatigue," the agreement of which with Prof. Haughton's formula is so "complete and satisfactory." The *lifting* of the weight was done in precisely the same way.

Domestic Economy of Blackbirds

TWO Blackbirds having built their nest in full view of my bedroom window, I have been much interested in watching the process of feeding their young, &c. The cock bird is the principal forager, and the food generally brought are worms. My object in writing is to draw attention to one feature which may be unknown to most of your readers as relates to the disposal of the young birds' droppings. If left in the nest, it would become filthy, if thrown aside the accumulation would lead to detection, and I believe the general impression is that the old birds carry the soil away; but on watching them closely I never saw the droppings carried away but on one occasion, and that by the hen; in every other instance after being fed, the young birds in turn lift up their tails and the droppings are taken by the old bird and actually swallowed. On the 15th July the young birds being fully fledged, were literally washed out of their nest by the downpour of rain on that day, but, with a little care, they all survived. On the 22nd the hen again returned to her nest, and she is now sitting closely on three eggs, and I hope to get the next brood photographed. I enclose my card and address, and should any readers of NATURE desire to witness what I have described, I shall be very glad to afford them an opportunity.

Woolwich Common, 2nd Aug., 1875 E. R. W.

Scarcity of Birds

MR. BARRINGTON, writing from the Co. Wicklow, in NATURE, vol. xii. p. 213, says that he finds Blackbirds and Thrushes unusually scarce this year. I have not heard of this anywhere else, and certainly it is not the case here.

Old Forge, Dunmurry, Co. Antrim, July 26 JOSEPH JOHN MURPHY

Hay Crops of 1875

LET me record in NATURE the extraordinary fact that on Monday, July 26, in one of my meadows here, the first crop was carried while the second crop, or after-math, was being cut.

Valentines, Ilford C. M. INGLEBY

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The last number of *Vierteljahrsschrift der Astronomischen Gesellschaft* (x. Jahrgang, weites Heft), received within a few days, contains an ephemeris of most of the known variables, including those of short period, for the year 1876, drawn up by Dr. Schœnfeld, chiefly from the data in his catalogue of 1875. This early publication will, no doubt, be very acceptable to observers who are devoting attention to these interesting and puzzling objects.

THE GREAT CLUSTER, MESSIER 11.—As the first special publication of the Observatory of Hamburg, we have Prof. Helmert's memoir detailing the results of his micrometrical observations on the components of this well-known cluster in the constellation Aquila, or in *Clypeum* or *Scutum Sobieski*, as many of the Continental astronomers continue to call that part of the heavens in which it is situate. The memoir has a particular interest from the circumstance of Dr. Lamont having similarly employed the Munich refractor in the years 1836-39. The investigation of any changes that may take place in the constituents of these groups of stars, as regards position or brightness, becomes a very attractive one, and as we know from the excellent work of Herr Pihl on the Perseus cluster, it is not one always requiring the use of large instruments, such as have been employed in the hands of Lamont and Helmert, upon Messier 11. D'Arrest terms this cluster "magnifica innumerabilium stellarum coacervatio"; the amateur will remember Admiral Smyth's comparison of the configuration of the components to "a flight of wild ducks."

NEW MINOR PLANET.—No. 147 of this group was detected by Herr Schulhof, at the observatory of Vienna, on July 10, in the vicinity of β Capricorni. It is of the twelfth magnitude, and Prof. Littröw, the director, proposes to call it "Protogeneia," perhaps in allusion to it being the first minor planet discovered at this observatory. It may be presumed that he has satisfied himself of its distinctness from any of the minors which are now adrift.

THE GREAT COMET OF 1843.—The elements of the orbit of this remarkable body, finally derived by the late Prof. Hubbard, of the Naval Observatory, Washington, after a very masterly discussion of the whole series of observations, are as follow:—

Perihelion Passage 1843 February	27·41051	G.M.T.
Longitude of Perihelion ...	278° 40′ 17″	M. Eq. 1843.
„ Ascending Node	1 14 55	
Inclination of Orbit	35 40 39	
Excentricity	0·9999915717	
Perihelion Distance	0·0055383	

Motion—retrograde.

From which we have the following additional figures:—

Mean Distance from the Sun ...	65·711
Aphelion Distance	131·42
Period of Revolution	532·7 years.

The distance from the sun at the perihelion is less than that of any other comet so far computed; the famous comet of 1680, according to Encke's definitive calculations, making also a very close approach, though not so near as in the present case. If Leverrier's semidiameter of the sun be adopted, with 8″·875 for the solar parallax, we find—

Sun's semi-diameter	428,710 English miles.
Comet's perihelion distance ...	510,140 „

Whence it would appear that a little before 10 P.M. on February 27, the comet passed within 81,500 miles from the sun's surface, and if we compute the orbital velocity at the time, we find it 348·5 miles per second. The comet was less than $2\frac{1}{4}$ hours on the north side of the ecliptic, passing from ascending to descending node in 2h. 13·4m.

On examining with the above elements the track of the comet on the day of perihelion passage, it results that a transit over the sun's disc must have taken place at the descending node, the ingress (geocentric) occurring at 11h. 28m. Greenwich time, 241° from the sun's N. point towards E., and the egress at 12h. 29m., at 187° similarly reckoned. The transit might have been observed in Australia; the times for Sydney being, Feb. 27, 21h. 33m. for ingress and 22h. 34m. for egress. Such a transit brings to recollection an observation recorded in the Paris Astronomical Bulletin at the time as having been made by M. Aristide Coumbary at the observatory of Constantinople, on the morning of the 8th of May, 1865, from which it would appear that a dark spot moved over a space of 21′ upon the sun's disc, in a little over three-

quarters of an hour. From the data published by Coumbary we might infer on the hypothesis of circular motion, that the body, whatever its nature, had moved at a distance of about 415,000 miles from the sun's surface, and as we know from the experience afforded by the great comet of 1843, there is nothing improbable in a comet having so passed. Perhaps when the sun's disc is more systematically and widely watched, a comet may be caught in transit and properly observed. The case of the comet of 1819 is not a satisfactory one, Pastorff's observation at least attributing to it a position upon the sun's disc which it could not have occupied at the time he assigns to his observation.

COMET 1874 (II).—The comet detected by M. Coggia at Marseilles on April 17, 1874, which presented so fine an appearance in our northern heavens in July, was observed at Melbourne, and by Mr. Tebbutt, near Sydney, until the end of the first week in October. Comparing the Melbourne observation on the 6th of this month with the place given by the elliptic elements of Prof. Tietjen, the difference is found to be less than a minute of arc, and the European observations to the middle of July are very accurately represented by these elements. Between April 17th and October 6th the comet traversed an arc of 205° of true anomaly, and the near agreement of Prof. Tietjen's orbit throughout, shows that the comet when it attracted so much attention was really moving in an ellipse of very long period, though no doubt this element may be considerably varied without largely increasing the differences between calculation and observation. The period of revolution in Tietjen's ellipse is nearly 9,000 years. When a similar complete investigation has been made for this comet to that so skilfully performed by Dr. von Asten in the case of Donati's great comet of 1858, some kind of limits may be assigned to the time of revolution, but in all probability it must extend to some thousands of years. We remark that the Melbourne observations of Coggia's comet were made with a telescope of only 4½ inches aperture; no doubt the comet might have been followed some time longer with larger instruments, but it is possible that the Melbourne reflector may have been under preparation for the transit of Venus, and not conveniently available for cometary observations.

PROF. LOOMIS ON THE U.S. WEATHER MAPS *

THIS paper is in continuation of a similar paper published in July last year, in which the American Weather Maps for 1872-73 were discussed. The results then arrived at are compared with the observations of 1874, and the whole is thereafter combined into a three years' average.

The principal conclusions from the three years' observations are these:—

The mean direction of the onward course of storms is N. 81° E., or a little to the north of east, being most southerly in July (E. 7° S.), and most northerly in April and October (N. 72° E. and N. 74° E.). The mean velocity is 26 miles per hour—the maximum monthly velocity, 32 miles, being in February, and the minimum 18·4 miles in August. As regards particular storms, wide deviations from these figures take place, it being found that the actual motion of the storm's centre may have a path in any direction whatever, and the velocity of progress may vary from 15 miles per hour towards the west, to 60 miles per hour towards the east. From the tri-daily observations it is found that the average velocity of storms from 4·35 P.M. to 11 P.M. is about 25 per cent. greater than for the rest of the day, and that while this varies in different months from 14 to 32 per cent., the most rapid progress occurs in every month during this portion of the day. Prof. Loomis suggests that as this is the time of the day when the temperature is falling most rapidly, the fall of rain may be thereby accelerated, and the velocity of the storms' progress be increased by the more rapid extension of the rain-area which would follow. The meteorological system of the States fortunately furnishes the required data for the examination of this important point, and we shall look forward with great interest to discussions of the daily rainfall of the States in this connection.

It would appear that an unusual extension of the rain-area of a storm is generally accompanied by a velocity of progress greater than the mean. The average extent of the rain-area eastward from the centre of the storm is 542 miles; but when the eastern extent of this area is 100 miles greater than the mean, the hourly velocity of the storm's progress is increased 13¾ miles; and when on the other hand, the eastern extent of the rain-area is 100 miles less than the mean, the hourly velocity of progress is diminished 9½ miles. Whilst the extent of the rain-area exercises an important influence on the storm's progress, the inclination of its axis would also appear to influence to some extent the course of the storm. Professor Loomis is of opinion that the direction and velocity of the storm's progress may be predicted with some confidence, in cases when the precise limits of the rain-area are known. It is thus most desirable that rain observations form an integrant part of all weather telegrams.

The influence of areas of high barometer on the velocity and direction of a storm's course is important in connection with the prediction and theôry of storms, but further observations are required for its elucidation, among the more important of which are the movements of the upper currents of the atmosphere as disclosed by observations of the cirrus cloud.

The reports of General Myer, Chief Signal Officer, for 1872-73-74 show by the barometric results for Denver and the other elevated stations on the spurs of the Rocky Mountains, that the relative distribution of atmospheric pressure at these great heights is just the reverse in summer and winter of what obtains at lower levels to eastward in these respective seasons. The point is a vitally important one in its bearings on the weather and meteorology of the States. In connection with it, we have examined with much interest the tables at pp. 10 and 11 which give the number of times during 1873 and 1874 on which the daily change of temperature amounted at the different stations to 40° and upwards. This large temperature fluctuation occurs most frequently at Colorado Springs, Denver, and the other high stations in the west. The most remarkable of these changes occured at Denver on the 14th of January 1875, at which place the temperature was below zero all day, and the wind N.E. At 9 P.M., the temperature was 1°.0 and the wind suddenly shifted to S.W.; at 9.15 P.M., the temperature had risen to 20°.0 at 9.20 P.M. to 27°.0; at 9.30 P.M., to 36°.0; and at 9.35 P.M., to 40°.0, after which there was little change till the following morning. At 11.30 A.M. of the 15th, the temperature was 52°.0 and at this time the wind suddenly backed to N.E.; at 12.30 P.M., the temperature had fallen to 4°.0 Thus in the evening of the 14th, the temperature rose 39°.0 at Denver in the short space of 35 minutes, and about noon of the following day fell 48°.0 in one hour.

* Results derived from an examination of the United States Weather Maps for 1872-73-74. By Prof. Elias Loomis, Yale College. From the *American Journal of Science and Arts*, vol. x., July 1875.

ON THE HORIZONTAL PHOTOGRAPHIC TELESCOPE OF LONG FOCUS *

IN what I have now to say in regard to the methods of Photography employed in observing the recent Transit of Venus, I shall confine myself to the subject of

* This paper was read by the late Prof. Wenlock to a private scientific Club in Cambridge, U.S., shortly before his death; it has been forwarded to us for publication, at the request of the Club, by Prof. Asa Gray.

the instruments used, having especially in view the explanation of the advantages of the horizontal telescope and its origin.

I should not have thought it worth while to make any communication on this subject, but since it has been a matter of discussion in the French Academy, and several pamphlets have been written on the subject, it may not be uninteresting to explain here the connection of the Observatory of Harvard College with it.

In the spring of 1869, when it became necessary to begin preparations for observations of the Solar Eclipse of August 7th of that year, my attention was called especially to the subject of Solar Photography for the first time. Mr. Warren De La Rue in England, and Mr. Rutherford in America, had devoted themselves almost exclusively to astronomical photography for many years, and they were the authorities on this subject. The methods employed by them were substantially the same; each used an equatorial telescope with clock movement, and enlarged the image formed by the object-glass by means of another system of lenses, and photographed this magnified image. Mr. De La Rue corrected his object-glass to secure as nearly as possible the foci of chemical and visual rays, and Mr. Rutherford corrected his so as to obtain the best echromatic combination of the chemical rays without regard to the visual focus.

Mr. De La Rue first undertook a series of daily photographs of the sun at Kew some time previous to 1860. In making my own preparations for the Solar Eclipse of 1869, I collected what information I could about the extent and brightness of the corona, and the nature of Mr. De La Rue's photoheliograph which he employed in observing the total eclipse of 1860 in Spain. In the first part of the volume of "the "Philosophical Transactions" for 1869, I found a paper which was read May 31, 1868, which contains the results of the first attempt to measure the heliographical positions and areas of sun-spots observed with the Kew photoheliograph. From the examinations of the measurements in this paper I became convinced that no trustworthy measures of photographs taken in this way could be made. The magnified image is so much distorted by the eyepiece, or the equivalent system of lenses used to form the image in the camera, that no satisfactory scale could be obtained; in fact the scale was found to vary irregularly from the centre to the circumference of the image; and even if this irregular scale could be investigated, a slight displacement of the centre of the picture from the axis of the telescope would introduce confusion. Mr. De La Rue's method of investigation consisted in photographing the pinnacle of a pagoda which was composed of rings and chains of known dimensions, and then attempting to find the scales of the different parts of the pictures from the images of this pinnacle.

The result, as I have said, satisfied me that this was a method to be avoided. The difficulty arising from the distortion of the image, and the apprehension that the light of the corona might be so enfeebled by enlargement that it would not make an impression on the plate, determined me to photograph the image in the principal focus of the object-glass.

All of the many other parties fitted out for photographing the total phase of this eclipse followed the method of De la Rue and Rutherford; the expedition from the Observatory of Harvard College was the only one that succeeded in getting a picture of the corona. The method of De la Rue was employed in the preceding eclipse of 1868, and no photograph of the corona was secured. I mention these facts simply to show how little the disadvantages of enlarging the image by an eye-piece were appreciated. In the next eclipse no party went into the field with De la Rue's plan; every one of them photographed in the principal focus, but this time, on account of the weather, the American party in Spain alone succeeded in getting the corona.

In 1871, in India, this method was again followed by all the parties, and was successful. My preparatory experiments in 1869 were made with an equatorial of 7 feet focal length, which gave an image of about three-fourths of an inch, and with the great equatorial of 24 feet, which gave an image of $2\frac{1}{2}$ inches diameter.

Measurements of photographs of the smaller image seemed to indicate that under a microscope an accuracy comparable with that of the best meridian circles was attainable; but believing that a larger image would be better, I thought that four inches would be a convenient size. In order to get such an image free from the distortion of the eye-piece, I must have a telescope 40 feet in length. Immediately on my return from the eclipse of 1869, I ordered a lens of Messrs. Clark and Sons, of 40 feet focus, and a micrometer capable of measuring conveniently an image of four inches diameter. Thus the long telescope was adopted to escape the distortion.

Then of course the difficulty of mounting and handling a telescope of this length, especially when extreme precision in measuring was the main object, naturally presented itself. To obviate this I resorted to the very simple expedient of placing the telescope horizontally, so that it need not be moved at all, and reflecting the light of the sun through it by means of a plane mirror. This seemed likely to meet all of the difficulties of the case; the well-known methods of mounting and directing collimators rendered the utmost degree of accuracy attainable in directing such a telescope, and by putting the object-glass on one pier and the camera on another, using a tube which should touch neither, only for excluding the light, all disturbance of the focus by the expansion of the long tube was avoided.

Other information obtained by my preparatory experiments had an important bearing upon my plan at this time. I had found how difficult it was to get an exposure of the plate short enough. It became necessary to reduce the apertures of the equatorials to one or two inches, and then throw the slide across with a strong spring. From this I derived two important suggestions: one, that a heliostat driven by clockwork was not indispensable, as the picture would be instantaneous, so that the motion of the sun during the exposure would be of no consequence. The other was that I might reduce the light by using a transparent glass reflector, and not be compelled to reduce the aperture of the telescope so much. By these means the cost of the experiment was greatly reduced, saving the expense both of a heliostat and of a silvered mirror. Messrs. Clark and Sons did not get the apparatus ready for use at the Observatory until July 1870, although it was tried at their shop previously. A series of daily photographs was begun with it, July 4, 1870, and has been kept up with little interruption to the present time.

At this time and for a year or two after, I had not heard of this method being thought of by any one. No one of my acquaintances seemed to have any knowledge of any other claimant of the method. Mr. Rutherford, with whom I had frequent communication, and who had been occupied with the subject for twenty years, regarded it as new and original. It was described in Mr. Lockyer's paper in 1870, and attributed to me; Mr. Newcomb, in the latter part of 1872, speaks of it as a method devised by me, and in successful operation for several years, and also independently proposed by Faye. Lord Lindsay adopted it for his expedition to the Mauritius. Mr. De La Rue in several communications down to 1873 spoke of it as the method of the American Astronomers. It was afterwards, about this time, called the method invented by Foucault and Prof. Winlock independently. Then, in 1873, I received a book by M. Edmond Dubois, claiming it as a French

invention, and giving the whole credit to Capt. Laussedat, closing with the remark that if it should be successful the glory would belong to France. Afterwards I received a pamphlet of twenty-six pages, by Capt. Laussedat, in which, ignoring me entirely, he tried to sustain his claims against those of Faye, Foucault, and Fizeau.

In 1873, after the horizontal telescope had been in successful operation for three years, after specimens, both negatives and lithographic copies had been distributed in Europe, the French Commission, which had up to this time been making their preparations to use the method of de La Rue, adopted the horizontal telescope. It would appear from this, that whatever might have been done or said on this subject by the Frenchmen named above, it had not contributed much to a clear appreciation of the advantages of the method until after they had been demonstrated here.

Without caring anything about credit for priority of suggestion in such matters, being satisfied that no similar instrument was in use or had been used before the one at Harvard College, I was yet interested enough in the matter to look up the claims put forth by these gentlemen, and to see why they happened to be overlooked for so long a time, even by their own countrymen. I find that credit is accorded to Foucault, mainly for his perfection of the heliostat, both for the plane mirror and for the uniform motion. He published nothing in regard to its application to photography. After his death his friend, St. Claire Deville, spoke of it as one of the things that Foucault intended to do. He at the same time contemplated the use of the siderostat in all kinds of astronomical observations. M. Laussedat is unwilling to give him any share of credit for the horizontal telescope. M. Faye gives him credit only for the heliostat.

M. Faye himself took some photographs of the sun with a very long telescope of one M. Porro, of 15 meters focal length. The telescope was pointed directly at the sun. M. Faye's remarks on them before the Academy related only to the advantage of their size and their distinctness. He had nothing to say about the peculiar advantages of the long telescope, but he anticipated all succeeding inventions in the application of Photography to astronomy by predicting its early use in meridian and every other class of observations.

His next communication on this subject was on March 14, 1870, on the occasion of presenting a letter from M. Laussedat on the subject of a horizontal telescope. This was six months after my apparatus was ordered, and after some experiments had been made with it. In this communication he appears at first glance to have suggested the whole arrangement now adopted; but on closer examination he does not seem to have had any clear ideas about it. He recommends the use of a long telescope because he had seen good pictures with a long telescope; he nowhere speaks of his reasons for dispensing with the eyepiece, and in fact it does not clearly appear that he did dispense with it. In September, 1872, after it had been in use for two years and several accounts of it had been published, in his comments before the Academy on a paper of Warren De la Rue's, he seems to have understood for the first time the true theory of the long horizontal telescope.

Capt. Laussedat appears to have the most substantial claim of any that have been mentioned thus far. He used a horizontal telescope in Algeria in 1860, in observing the total eclipse of that year; but he used a very short telescope and had an eyepiece to enlarge and distort the image. His own account of what led him to this method was that he had no equatorial mounting for his little telescope and that no means were furnished him to buy one, but he had a good heliostat, and he resorted to the method as a makeshift. He fully appreciated, however, the advantages over the other method in the accuracy of orientation and in the certainty with which fixed lines of reference could be had on the plates.

M. Faye, in his communication of Sept. 1872, seriously claims that his use of the long telescope pointed to the sun in 1858—because M. Porro happened to have one, and Capt. Laussedat's use of a short one, placed horizontal, because he had no equatorial stand and clock movement—together make up the invention of the telescope as it is now used.

But, after all that has been said about the priority of suggestion, that question is settled finally by some one * in England finding that the wole arrangement was suggested by Hooke in 1676. A late communication on the subject in the *New York Times* calls it a method suggested by Hooke and perfected by Foucault.

In Hooke's day they had none but very long telescopes, but they had no heliostats. No practical application of his suggestion, however, seems to have been made.

ON THE CARDIOGRAPH TRACE

BY placing the sphygmograph, as constructed by M. Marey, over that portion of the chest where the heart can be best felt beating, instead of on the wrist-pulse for which the instrument is constructed, tracings called cardiograms can be obtained which bring to light physiological facts not otherwise ascertainable. In the last-published volume of the Guy's Hospital Reports there is a paper by Dr. Galabin, on the interpretation of these tracings, which will be read with interest by physiologists on account of the considerable difficulty there is connected with all attempts to explain the numerous ups and downs which they present between any two pulsations of the heart, and also because of the comparatively slight attention which they have had paid to them.

Dr. Galabin, in the paper under consideration, limits his observations almost entirely to the vertical variations in the curves under consideration, paying but little attention to the differences in the relative lengths of systole and diastole which they so clearly indicate, and which cannot be recognised with any degree of accuracy by any other means at our disposal. From a study of the cardiograph trace, he is led to the conclusion that the two most important elevations in the systolic portion of each curve are produced by the muscular movements in the heart itself, because "the more the heart is hypertrophied (by disease) the more prominent in comparison do these two become," and under these circumstances, "the effect of any oscillations, either of the blood or of any solid structures, would become less noticeable in proportion." It is remarked that "Marey's figures (of tracings indicating intracardial pressures) prove that the first, at any rate, of the cardiac impulse is not due to any stroke against the ribs caused by locomotion of the heart as a whole, which could only commence after the opening of the semilunar valves," because "the aortic valves do not open until the ventricular pressure has nearly reached its first maximum." It must, however, be noted that other tracings, obtained by the same illustrious physiologist, demonstrate equally clearly that the maximum of intracardial pressure is reached some appreciable time before the first major systolic cardiograph rise in the trace from the chest-wall, so that it may still be reasonably argued that the rise referred to depends upon the locomotion of the heart *en masse*.

To explain the second main systolic rise, Dr. Galabin makes a statement which needs considerably more demonstration before it can be considered to be proved. He refers to "inverted tracings," by which are understood curves in which all the rises in an ordinary trace are represented by depressions, in such a way that "to see more clearly their correspondence with positive tracings

* A correspondent in NATURE.—ED.

they should be turned upside down and read from right to left," instead of from left to right. Are we to believe, on the simple dictum of Dr. Galabin that *inverted tracings*, as above explained, are developed; that every elevation in the apex cardiograph trace is the result of a movement which is represented by a fairly proportionate fall in a trace a little distance from that spot; that every apical propulsion is a lateral suction? This may possibly be the case, but it requires a considerable amount of proof before it can be accepted as true. The relative duration, or, in other words, the horizontal projections of the different undulations, is not in favour of the assumption, which seems to be based on an accidental similarity between that apex trace and the reversed one from its neighbourhood. Till Dr. Galabin introduced his view, it has been assumed that the negative trace differs from the other positive trace in the fact that in the latter some of the undulations are longer in the up than in the down stroke; whilst in the former the reverse is the case. There is need for positive disproof of this explanation before the other is even considered.

Dr. Galabin concludes that the second main systolic rise "corresponds in time to the maximum contraction of the ventricle," and that it is due to the locomotion of the heart, dependent on the consequent injection of the aorta and the propulsion of the blood. This explanation might be tenable were it not for the results obtained by the employment of the hæmodromometer of Chauveau, tracings taken with which can be found in Marey's "Circulation du Sang" (p. 273). These show that there is a regurgitant current in the carotid arteries for some appreciable period *before* the closure of the aortic valve, which can only exist in connection with a similar one in the ventricular cavity. It is the hæmodromometer trace which has led the writer of this article to lay more than usual stress on the interval between the termination of the cardiac systole and the moment of closure of the aortic valves, termed by him the diaspasis.

Dr. Galabin remarks, "Mr. Garrod attributes the elevation *d* (the first main systolic rise), solely to the locomotion of the heart caused by the lengthening of the aorta. The rise *f* (the second main rise) he considers to intervene between the end of systole and the closure of the aortic valves, and to be due to the initial relaxation of the ventricle. It appears to be impossible that the relaxation of the ventricle, apart from its repletion, could produce an elevation in the curve except in those cases in which its hardening produces a depression either at the commencement or towards the conclusion of systole." In the explanation here referred to the elevation under consideration is, however, not supposed to be the result of the relaxation of the muscular walls of the ventricles, or to have anything to do with that phenomenon, but to be caused by the reflux of blood from the aorta and pulmonary artery into the ventricles which, when it has attained a sufficient velocity, closes the semilunar valves.

Dr. Galabin, by employing the stethoscope in conjunction with the cardiograph, watching the development of the trace whilst listening to the heart-sounds, has been able to satisfactorily verify the observation that the first sound occurs during the primary up-stroke, and that the instant at which the second sound is heard corresponds to a point on the principal down-stroke, and before the succeeding small and constant rise. This is further verified by the superposition of the sphygmograph trace on the cardiograph trace taken at the same time, a method which has elsewhere been shown to lead to particularly important theoretical results.

No particular stress is laid by Dr. Galabin on the peculiarities of the cardiograph trace associated with different rapidity of pulse and nothing else. The thorough study of the subject necessitates this point being taken into consideration, as is demonstrated by the great differences there are always found in the curves derived from the same individual when the heart beats at say 45 and 125 a minute.

Most of the paper under consideration is devoted to pathological points, especially mitral stenosis or contraction. With this we cannot here deal. One particularly interesting tracing proves that in some extremely slow pulses (*e.g.* twenty-five a minute) there may be an abortive attempt towards an intermediate contraction, perceptible in the cardiograph tracing, but not seen in that from the arterial pulse.

Whilst on this subject it may be mentioned that Dr. C. Hanfield Jones has recently read a paper before the Royal Society on reversed sphygmograph tracings, or tracings in which the systole is represented by a fall instead of a rise. These he explains on the assumption that they are produced by the brass end-pad of Dr. Sanderson's modified instrument resting on the artery instead of the spring-pad. This is no doubt the true cause in many cases; these tracings are, however, in our experience sometimes produced when Marey's unmodified instrument is employed. They may sometimes result from the fact that a curved artery is, during systole, rendered part of a larger curve, and so slips from under the spring-pad at that time.

A. H. GARROD

SIR JAMES KAY-SHUTTLEWORTH ON SCIENTIFIC TRAINING

ON the occasion of presenting the prizes to the successful students at the Giggleswick Grammar School, near Settle, on July 28, Sir J. Kay-Shuttleworth made some forcible remarks on the above subject. Sir James points out with so much wisdom the relative position which science and literature ought to hold in the training of youth, that his remarks deserve the serious attention of all interested in education. Our columns constantly bear witness to the increasing prominence given to science in education, both at the higher schools and universities. Sir James, after noticing this and other features in the progress of the Giggleswick School, and referring to some of the results of the training of the school, went on to say:—

"You will perceive that among them are proofs of the influence of the practical teaching in natural science in opening a career to our pupils in the universities. In the growth of any institution on a new basis, time must be allowed for its development. Difficulties will be encountered in discipline, in domestic management, and in the attainment of the ideal to which its course of studies is expected to rise. Yet it is well to keep that ideal closely in view as the goal of all efforts; to retain a firm hold on the principles of action, and while confessing the length and the arduous character of the way, to press forward, undismayed by any partial failure, towards the summit of our hopes. I find in the examination papers a continually higher standard. They embrace a wide range of studies. But it must not be supposed that we are so presumptuous as to expect that even the *élite* of the school could attain a high degree in the whole range of these studies. No error could be more fatal than that they should be obligatory on all our pupils. Indeed, we must, in the first place, point out that in consideration of the prominence given to modern languages and to practical instruction in natural science, Greek is not among the subjects comprised in the scheme of the school, though it will be taught to all boys preparing for the universities, or for any of the public examinations. To determine how best the faculties of those not gifted with average energy and capacity can be developed requires a delicate and thoughtful discrimination. But the curriculum is open to boys in proportion to the mental and physical vigour which they bring to the task. I have said that Greek is not one of the subjects of instruction made obligatory by the scheme, and the reasons for this will become more apparent as I proceed, but among these reasons is

no want of appreciation of the ancient classical literature, or of the classical languages as means of mental culture. It may, therefore, be desirable to say that we appreciate the treasures bequeathed to us by them in philosophy, poetry, history, and art, and in the principles of jurisprudence." After speaking in high terms of the value of the classical languages as pedagogical instruments, Sir James went on :—

"But while we thus emphatically express our sense of the value of the classical languages as instruments of mental training and sources of the highest literary culture, the curriculum of this school includes pure and applied mathematics. These studies, which stretch back to the period of Greek civilisation, have grown with the development of astronomical and physical research. They are the instruments of the abstract investigation of physical laws. But we have also sought to place the school practically in relation with natural science. The question has been much discussed whether science should be thus taught through the whole school course, or whether it should be interstratified with the other studies. We shall endeavour to solve these questions by introducing in the junior forms the cultivation of the faculties of observation by the practical study of botany and physical geography, for both of which this neighbourhood affords considerable opportunities. For somewhat more advanced students we have built a good chemical laboratory, and we are about to extend this building so as to provide separate rooms and apparatus, and for the practical study of experimental physics. The thorough knowledge of any branch of experimental science involves an acquaintance with the instruments and modes of investigation, as well as skill in manipulation. These are not to be acquired from books. It is indispensable that pupils should become familiar with the phenomena of the operation of natural forces. They must learn to observe, to practise the philosophy of induction by following the footsteps of the great masters of research in preparation for independent efforts. The faculties exercised in such pursuits are not altogether the same as those employed in literary studies. They may be compared without the depreciation of either. The student of literature has opportunities to cultivate what is metaphysical—whatever relates to art, to poetry, to history, philosophy, or language; while the student of nature may more successfully develop the faculties of observation and those brought into play in the processes of inductive and deductive reasoning. The search for hidden truths trains the ripe student in habits of scrupulous exactitude. To record such observations is an exercise in accuracy of thought and language.

"The scientific habit of mind which is the result of a thorough practical training in one or more branches of science is not to be attained by any devotion to language or literature, just as the development of taste in literature, or of critical skill, or of the power of philological research and discovery cannot be gained in the laboratory. These distinctions between literature and science are in harmony with the diverse capacities of boys, and they may be employed as auxiliaries in the development of boys of limited or one-sided capacity. Some pupils who have low grammatical and linguistic power may yet exhibit facility in mathematical processes. Others in whom both these faculties are feeble, awaken to intellectual life as observers of nature. To some minds the facts and principles of science become easy only when they are in contact with the actual phenomena. Hence one part of the value of practical studies in the field and the laboratory. It may be confidently asserted that when any of these classes of mental power is feeble, the development of that part of the brain which is most easily awakened to activity will communicate vigour to the rest; the whole brain will become more healthy and active. A boy incapable of successful literary effort, but who has power as an observer, may, by that form of mental culture, by-and-by become more capable of literary application and success. Thus the literary, the mathematical, and the practical scientific studies of schools become, in the hands of a thoughtful and skilful master, preparatory or co-ordinate instruments of mental development.

"There has been of late a new era in the development of the natural sciences. This commenced with the discoveries of great mathematicians and astronomers, and extended to every department of physical research. After Kepler and Newton, mathematics in their application to experimental physics and astronomy established themselves, especially at Cambridge, as a prominent part of the studies of the European Universities. But during the present century, the rapid development of every department of natural science has created new claims for the introduction of new courses of study, for which the universities are gradually increasing their means and appliances, and towards the successful cultivation of which they are extending their honours and rewards. What happened at the revival of learning with respect to the classical literature is about to happen in the fuller recognition in the universities of every department of natural science. The Chancellor of the University of Cambridge has recently munificently founded a physical laboratory in that university. Certain of the colleges have established chemical and biological laboratories. The Geological Museum lectures and fieldwork continue to develope. These are preliminary steps towards practical instruction in every department of natural science. At Oxford, the university has built an admirable museum, with which are connected laboratories for chemical and biological studies, and for those of experimental physics and geology. Certain of the colleges have also laboratories, and readers or demonstrators of practical science. The Commission on Scientific Instruction, which has just closed its five years' labours, has made many suggestions as to the facilities to be granted to students of natural science in both universities. For example, it recommends the freer admission of those who are successful to the honours of the university, as well as to the scholarships, fellowships, and government of the colleges. The Commission had such opportunities of ascertaining to what extent these recommendations expressed the opinions of the governing minds of the universities, that there can be no doubt that no insurmountable obstacle will be encountered in the establishment of studies in natural science in a position, in relation to their honours and rewards, which will duly represent the part which science has to play in the education of the country.

"The methods and results of natural science have now so far affected all our modes of thought that they claim their place in the arena of all forms of discussion. They must, therefore, also take their place in the studies of the public and grammar schools, and of the colleges and universities which would fitly train men for the work of life. It would be a grave disadvantage to this nation if its rulers in Parliament and in the Cabinet should represent chiefly literary culture, without a familiarity with the physical sciences. Such a result could not now long exist without a neglect of opportunities of promoting scientific culture and research, which would be injurious to the education of the country and prejudicial to the development of its material resources. Perhaps it would be a much graver misfortune if there should grow up in the country two forms of thought—one derived from the exclusive contemplation of the metaphysical, and the other resulting from purely physical and materialistic studies. Moreover, to a man of education, however ripe and complete may be his classical accomplishments, it must be a great misfortune to have had no training in the natural sciences. He must have a sense of partial development, and of the deprivation of a great source of mental pleasure. These are, doubtless, among the reasons why, in the great public schools, instruction in natura

science has recently been introduced by the appointment of skilled teachers, by the building of laboratories and the establishment of museums, and by the regulations of the commission of public schools as to the time to be allotted to such studies. Among our provincial grammar schools Manchester has provided laboratories and the means of highly skilled scientific instruction. At Burnley, also, laboratories have been built, and the head master, Mr. Hough, is distinguished by his scientific knowledge and practical skill. He, doubtless, will diligently employ the means at his command. The Commission on Scientific Instruction has carefully collected the experience of the schools which have introduced practical scientific teaching. They strongly recommend that such instruction should take its place at the side of that which is literary throughout the whole school course. We had practically anticipated this suggestion at Giggleswick. I do not prominently put forward the adaptation of such studies to the wants of the great manufacturing districts of Yorkshire, Lancashire and Cumberland, which are contiguous to us, or of the Durham and Northumberland coalfield. Yet many of the sons of wealthy men in these districts, as well as of those engaged in scientific professions, will complete their education at school. In these trades and professions the practical commencement of a scientific training is often of great value. As I have already said, it forms the scientific habit of mind; it familiarises the youth with the phenomena of the operation of natural laws, and with the manipulation of instruments. It developes the faculty of observation and the power of inductive and deductive reasoning. Moreover the facts of physical science learned in the laboratory are an invaluable possession to the engineer, the chemist, the miner, the physiologist, and to every professional man who has to use these facts, principles, and processes as a part of his daily occupation. This school is intended to offer, in the first place, a sound preparation in elementary knowledge in the English language, its grammar, composition, and some acquaintance with English history and literature. Within the range of its studies are the ancient classical literature and modern languages. It would fail in its purpose if the humble elements of arithmetic were not faithfully cultivated as the basis of mathematical knowledge and scientific calculation. It is on this broad basis that we wish and hope to rear the structure of a sound and scientific culture.

"The questions which the governors of this school have attempted, through years of patient labour, to solve, are also awaiting solution in all similar schools. What are in future to be the relative positions of the literary and scientific education of our youth? How, as in this school, can the financial resources be developed so as to provide laboratories, and a larger skilled staff of teachers, in order to ensure a sound literary culture, together with scientific instruction? Inseparable from these questions is the formidable one, Whence are the skilled teachers of science, capable of giving practical instruction in laboratories to be provided, if science in this sense is to form part of the curriculum of all schools? Where the income of the school is small, that difficulty is at present insurmountable, for a separate science master cannot be afforded in such schools. Nor will it be removed until some means be devised for the training of teachers by which they will be enabled to add practical skill in scientific instruction to a sound basis of literary culture. Then a single master may fulfil the double function in a school. The commission on scientific instruction points to this, among many other reasons, for the establishment, within the universities and elsewhere, of a system of training for masters of schools above the elementary in the art and practice of teaching, and in a practical knowledge of science. The governors of this school of King Edward the Sixth of Giggleswick have not been negligent of the bearing of their labours on these wide general questions. So far as they have proceeded, they are satisfied that a sound literary culture may not only subsist with practical instruction in science, but that, under earnest and thoughtful guidance, these departments of instruction may each contribute to the intellectual activity and to the success of every form of teaching in the school."

THE INTERNATIONAL GEOGRAPHICAL CONGRESS AND EXHIBITION

THIS Congress, which has been looked forward to with considerable expectation, was opened in the Salle des Etats of the Tuileries, on Sunday last, in presence of the President of the French Republic, many of the dignitaries of State, foreign ambassadors, and other eminent persons. There was a large attendance of the general public, and addresses were given by the President of the Congress, Admiral de la Roncière le Noury, Baron von Richthofen, Sir Henry Rawlinson, and other delegates of the various nations represented at the Congress.

The regular work of the Congress commenced on Monday, and the sittings will be continued till the 11th inst., when a distribution of medals will take place. We believe a few prizes will be awarded to England, but not many, as our country has contributed but scantily to the exhibition. To-day a visit will be made to the Paris Observatory, and to-morrow one to the Historical Museum of National Antiquities (mostly pre-historic) at St. Germain.

Juries have been appointed to decide on the awards in the various sections of the Exhibition, and a notable feature of these is that not a single Frenchman has been appointed a president; this, we believe, is the result of characteristic delicacy on the part of the French authorities. Col. Montgomerie and Major Wilson are the English representatives.

The Exhibition continues to be well attended, and we hope the receipts will be sufficient to reimburse the Committee, who have become responsible for a large sum, the French Government and Geographical Society having contributed a very small amount.

In the English Section the books of photographs illustrating the people of India and China and the Chinese have proved very attractive. The photographs exhibited in the Russian annexe are very numerous, and relate to people of every tribe and condition inhabiting the empire. Austria has also been very successful in this respect, having exhibited photographs and drawings illustrating the chief incidents of the *Tegethoff* Polar Expedition.

A special room has been set apart for the several Alpine clubs, which have been created in imitation of the English Alpine Club. The publications of the parental association, and the scientific and other apparatus used in Alpine climbing by the English, French, and Italian clubs, are exhibited, and are inspected with evident interest.

The French Government exhibits the results of the missions sent out by the Ministry of Public Instruction. These have been numerous and successful. Independently of the Transit of Venus Expedition, we must mention a series of pictures showing the Bay of Santorin, in the several successive stages of creation of the new volcanic island. These illustrate happily how continents come into existence.

The Hall of National Antiquities (Pre-historic) is a compendium of the Saint Germain Museum, which will be visited by the Congress. A number of highly instructive maps, showing the distribution of relics of the Stone Age, Iron Age, &c., have been published, and are exhibited by the Historical Commission on the Gauls, which was created by Napoleon III. while writing his "Life of Cæsar," and will be continued for a lengthened period.

Amongst the real curiosities of the Exhibition, we must mention a microscopic photograph of the French map by the staff. This photograph was executed by M. Dagron,

the inventor of microscopic photographs for carrier pigeons during the war. The 250 maps, covering a space of more than a hundred yards square, are so reduced on glass, that they can be packed in a portfolio weighing half a pound when full, and examined with a small microscope with perfect facility and clearness.

M. Bouvier, a French naturalist, has presented a collection of almost all the known species of *Algæ* collected in the fish market at Paris.

NOTES

THE following are the officers of the forty-fifth meeting of the British Association which will commence at Bristol on Wednesday, August 25, 1875:—President-elect—Sir John Hawkshaw, F.R.S. Vice-Presidents-elect—The Right Hon. the Earl of Ducie, F.R.S., the Right Hon. Sir Stafford H. Northcote, Bart., F.R.S., the Mayor of Bristol, Major-General Sir Henry C. Rawlinson, F.R.S., Dr. W. B. Carpenter, F.R.S., W. Sanders, F.R.S. General Secretaries—Capt. Douglas Galton, F.R.S., Dr. Michael Foster, F.R.S. Assistant General Secretary—George Griffith, F.C.S. General Treasurer—Prof. A. W. Williamson, F.R.S. Local Secretaries—W. Lant Carpenter, F.C.S., John H. Clarke. Local Treasurer—Proctor Baker. The sections are the following:—Section A: Mathematical and Physical Science. President—Prof. Balfour Stewart, F.R.S. Section B: Chemical Science. President—A. G. Vernon Harcourt, F.R.S. Section C: Geology. President—Dr. T. Wright, F.R.S.E., F.G.S. Section D: Biology. President—P. L. Sclater, F.R.S. Department of Zoology and Botany, Dr. P. L. Sclater, F.R.S. (President), will preside. Department of Anatomy and Physiology. Prof. Cleland, F.R.S. (Vice-President), will preside. Department of Anthropology. Prof. Rolleston, F.R.S. (Vice-President), will preside. Section E: Geography. President—Major-General Strachey, F.R.S. Section F: Economic Science and Statistics. President—James Heywood, F.R.S., Pres. S. S. Section G: Mechanical Science. President—William Froude, F.R.S. The First General Meeting will be held on Wednesday, August 25, at 8 P.M. when Prof. Tyndall, F.R.S., will resign the chair, and Sir John Hawkshaw, C.E., F.R.S., President-elect, will assume the presidency, and deliver an address. On Thursday evening, August 26, at 8 P.M., a *soirée*; on Friday evening, August 27, at 8.30 P.M., a Discourse by W. Spottiswoode, LL.D., F.R.S., on "The Colours of Polarised Light;" on Monday evening, August 30, at 8.30 P.M., a Discourse by F. J. Bramwell, C.E., F.R.S., on "Railway Safety Appliances;" on Tuesday evening, August 31, at 8 P.M., a *soirée*; on Wednesday, September 1, the Concluding General Meeting will be held at 2.30 P.M. A special lecture to working-men will be given by Dr. Carpenter, F.R.S., on the evening of Saturday, Aug. 28; the subject will be "a piece of limestone." The Local Committee have done everything in their power to make the Bristol meeting a success. All the non-local sectional secretaries will be lodged at the Queen's Hotel, close to the reception-room, at the Local Committee's expense; this will no doubt conduce much to the easy working of the meeting. The experiment of a room for the exhibition of specimens and apparatus, tried first last year at Belfast, will be repeated this year. The President will be the guest of the Mayor, who will occupy for the first time the new Mansion House just given to the city by Thos. Proctor, Esq. Most of the other office-holders, as also all the foreign members, who have intimated their intention of being present, and several English members, have received private invitations from gentlemen resident in Bristol and neighbourhood. Many other hospitable arrangements have, we believe, been made, and altogether, so far as enjoyment and comfort are concerned, this promises to be one of the most satisfactory meetings of the Association. As we previously intimated, a specially prepared Guide, compiled by several gentlemen, will be published by Wright and Co., of Bristol; a lodging list with useful map will be issued this week. The whole of the Victoria Rooms, Clifton, will be used as a reception-room. All the evening meetings and *soirées* will take place at the Celston Hall, and satisfactory arrangements have been made for the meetings of sections. Several interesting excursions have been arranged for, including two to the Mendips, and handsome offers of entertainment have been made by those gentlemen to whose neighbourhood the excursions are to be made.

A NEW physical observatory is to be erected at Fontenay, the head of which will be M. Janssen. It will be erected on the very spot where it was intended to build one when it was proposed some years back to remove the Paris Observatory. In a few months, then, Paris will have four observatories—the National, the Physical, and two meteorological observatories—one at Montsouris under M. Marie-Davy, and another which is being built at the Acclimatisation Gardens. It is said that some members of the Municipal Council will propose to connect all these observatories with the National one by a special wire to register automatically all the meteorological observations by the Rysselberghe process, which we noticed last week in connection with the Geographical Exhibition.

THE Smithsonian Institute and the Indian Bureau are engaged in forming for the U.S. Centennial, a collection exhibiting the past and present history of the Aboriginal races of America.

"THE German Abyssinian Company."—A company has been incorporated in Berlin which proposes to found at Choa, the most southern province of Abyssinia, a permanent settlement, in order from thence to send out scientific expeditions into the unexplored portion of Africa, and to develop the commerce of the country. The objects of the Company are, however, supposed to be more commercial than scientific.

THE Khedive has issued a decree ordering the enforcement of the metrical system in Egypt from the 1st of January, 1876.

DR. HAWTREY BENSON, of Dublin, writing to the Dublin *Daily Express* under date July 27, describes a remarkable shower of small pieces of hay which he witnessed at Monkstown that morning. It appeared in the form of "a number of dark flocculent bodies floating slowly down through the air from a great height, appearing as if falling from a very heavy dark cloud, which hung over the house." The pieces of hay picked up were wet, "as if a very heavy dew had been deposited on it. The average weight of the larger flocks was probably not more than one or two ounces, and, from that, all sizes were perceptible down to a simple blade. The air was very calm, with a gentle under-current from S.E.; the clouds were moving in an upper-current from S.S.W." The air was tolerably warm and dry, and the phenomenon is thus accounted for by Dr. J. W. Moore: "The coincidence of a hot sun and two air currents probably caused the development of a whirlwind some distance to the south of Monkstown. By it the hay was raised into the air, to fall, as already described, over Monkstown and the adjoining district."

IN the Paris *Bulletin International* for July 30 last Prof. Raulin of Bordeaux gives the result of an examination of a comparison of the gross amount of the rainfall for the ten years 1851-60 with that for the ten years 1861-70, from which it is shown that, as regards the southern half of France, the rainfall during the former of these decennial periods exceeded that of the latter at forty-six out of the fifty-three stations at which observations were made for the twenty years. A similar distribution of the rainfall during these two decennial periods appears to have taken

place, with few exceptions, over a large area, embracing the British Isles, France, Germany, Italy, Spain, the basin of the Mediterranean, and Algiers. The point is an interesting one, and we hope that meteorologists will inquire how far the rainfall observations of their respective countries agree with the result obtained by Prof. Raulin for the southern half of France.

IN the *Journal* of the Scottish Meteorological Society for 1874-5, just published, will be found a long and elaborate paper by Mr. A. Buchan and Dr. Mitchell, on the Influence of Weather on Mortality from different diseases and at different ages; some of the results which have been arrived at will be found in an abstract by Mr. Buchan, which we publish to-day. Other papers in the *Journal* are on proposed portable Iron Barometers, and on a simple form of Anemometer, by Mr. T. Stevenson, C.E.; Meteorological Register at Inveresk for 1874; Table of Observations connected with the periodical return of the Seasons; Additional Rainfall returns for 1874; and Meteorological returns, with notes of the prevailing weather and state of vegetation at the Society's stations for the year; besides reports of the general meetings of the Society held on July 3, 1874, and February 10, 1875.

IT is expected that an important meeting of the Council of the Paris Observatory will be held to-day, a member of the Academy having been desired to explain his assertions relating to astronomical observations. The results will very likely be made public.

THE recent French inundations have recalled to memory an experiment which was tried twelve years ago before Napoleon III. The design was to manufacture mattresses of cork, so that any one on board a ship or in a house which could be flooded would have in his bed a ready-made raft capable of floating under a weight of more than 1 cwt. for any length of time. Cork is a material so soft that mattresses made of it are not inferior to any other for comfort.

A MEMORIAL in marble of Sir John Franklin was uncovered on Saturday by Sir George Back in Westminster Abbey. The monument has been erected by the late Lady Franklin, and contains some appropriate lines by Mr. Tennyson.

THE recent attack upon Lieut. Conder's Palestine exploring party occurred near Acre. Lieut. Conder was badly, but not dangerously, wounded.

THE election to the vacant Professorship of Medicine in the University of St. Andrews will take place on Sept. 22 next.

THE *Shearwater*, which was commissioned by Capt. Nares (now commanding the Arctic Expedition) on July 20, 1871, for surveying service on the Mediterranean Station, arrived at Sheerness on July 23 last. In Saturday's *Times* will be found a brief account of the work done by the ship during her four year's service. During part of the cruise in the Mediterranean Dr. Carpenter was on board to investigate the results of soundings and dredgings. Commander W. J. L. Wharton took command of the ship on Capt. Nares leaving to join the *Challenger*. After having been two years in the Mediterranean the *Shearwater* proceeded to Zanzibar, in order to survey the island and the opposite coast. In February 1874 the ship proceeded to the Cape of Good Hope, leaving Cape Town on July 14 with the Rodriguez Transit party. At Rodriguez the ship was constantly employed on work connected with the Transit, running meridian distances, surveying the island, and assisting the shore party in various ways. After landing the Transit party at Mauritius, the *Shearwater* again proceeded to Zanzibar to continue surveying work, officers and men, however, suffering greatly from fever. Zanzibar was left on May 8, and the *Shearwater* reached England as we have said on July 23 last. During the four years the ship has been in commission, she has surveyed in detail 790 miles of coast line and sounded closely over an area of 5,900 square miles. Most of the earlier surveys have been published.

IN the new part for May 1875 of Hoffmann's *Niederländisches Archiv für Zoologie* there are two papers of interest—one by Dr. A. A. W. Hubrecht, on the Nemertines of the Gulf of Naples, the other by Prof. P. Harting, on the eggs of Cyanea-Otoliths of Cyanea, and Chrysaora-nerve ring and organs of sense of Eucope-Chromatophores of the embryonic Loligo; being notes made during a stay at Scheveningue.

THE following candidates have been successful in the competition for the Whitworth Scholarships, 1875:—Joseph Harrison, 21, Mechanical Engineer; George Goodwin, 20, Mechanical Engineer; John Alldred, 21, Locomotive Engine Fitter; Franklin Garside, 20, Pattern Maker; Frank W. Dick, 21, Mechanical Engineer; Joseph Poole, 20, Fitter and Turner.

THE forty-third meeting of the British Medical Association opened on Tuesday in Edinburgh. Sir Robert Christison, the President, in his inaugural address, treated of the subject of Medical Education, entering into a complete history of the Medical School of Edinburgh.

THE twelfth number of Mr. Hermann Strecker's quarto work upon indigenous and exotic lepidoptera has lately been published by him at Reading, Pennsylvania, and contains, as usual, one plate filled with figures of butterflies. Among them are several very conspicuous forms, the most prominent being that called by him *Eudamonia Jehovah*, a term to which very serions objection has been raised on account of its apparent irreverence, but which he stoutly defends. Several new species are described; one of them being figured under the name of *Hepiolus thule.*

THE Report of the Agri-Horticultural Society of Madras, for 1874, shows that the Society is effecting much substantial benefit in its district, especially in the assistance and encouragement it offers in the introduction and cultivation of useful plants, that will thrive in the different climatal regions of Southern India, European vegetables, fibre-producing plants, coffee, tea, tobacco, indigo, &c. The cultivation and preparation of tea is strongly encouraged, and substantial prizes awarded for the best sample of different sorts. Some of the samples submitted to the brokers at Calcutta for their judgment are described as being of superior quality. A flower, fruit, and vegetable show is held annually, and it is worthy of note that all the prizes for ferns and other plants with ornamental foliage were gained by native gentlemen. There was last year a special class for the vegetable productions of native market gardeners, and the European vegetables exhibited are reported to have been of fair quality. The "list of new plants introduced in 1874" is remarkable for the very small proportion of correctly spelt names.

IN reference to Tidal Mills (vol. xii., p. 212), a correspondent writes that they have engaged the attention of Gregory, Barlow, Belidor, and Aldini, as will be seen on turning to the article on Tidal Mills in the "Penny Cyclopædia."

THE additions to the Zoological Society's Gardens during the past week include a Punjaub Wild Sheep (*Ovis cycloceros*) from Muscat, presented by Commander Yarforth; a Ruffed Lemur (*Lemur varius*), a Mongoose Lemur (*Lemur mongoz*) from Madagascar, two Rheas (*Rhea americana*), a West Indian Rail (*Aramides cayennensis*) from S. America, a Golden-naped Amazon (*Chrysotis auripalliata*) from Central America, two Yarrell's Curassows (*Crax carunculata*) from S.E. Brazil, two Razor-billed Curassows (*Mitua tuberosa*) from Guiana, deposited; a Short-tailed Muntjac (*Cervulus micrurus*), a Crested Pigeon (*Ocyphaps lophotes*), five Amherst Pheasants (*Thaumalea amherstiæ*), and six Japanese Pheasants (*Phasianus versicolor*), bred in the Gardens.

*THE MORTALITY OF THE LARGE TOWNS OF THE BRITISH ISLANDS IN RELATION TO WEATHER**

THE materials for this inquiry have been obtained from the Weekly Reports of the Registrars'-General for England and Scotland for the ten years, 1865–74. The data discussed embrace returns of deaths from all causes and at all ages, deaths of persons under one year of age, of persons above sixty years of age, and deaths from diarrhœa. The weekly averages have been calculated on the annual rate of mortality per 1,000 of the population.

The results for every one of the large towns show during the winter months an excess above the average mortality. A regards the English towns, that excess is greatest at Norwich, Wolverhampton, and Nottingham, and least at Bradford, Leeds, Salford, and most other towns in the north. In Scotland the winter excess is greatest at Aberdeen, and least at Leith and Greenock. At Dublin, the largest monthly mortality, 22 per cent. above the weekly average, occurs during February and March, being from a month to six weeks later than the time of the maximum of the English and Scottish towns.

In all the English towns, the minimum mortality of the year is in the spring months, the amounts below the averages of each town being greatest at Norwich, Wolverhampton, Birmingham, Leicester, and Nottingham. In Scotland, on the other hand, autumn is the healthiest season. In Glasgow and Edinburgh the deaths fall about 20 per cent. below the average in the month of September.

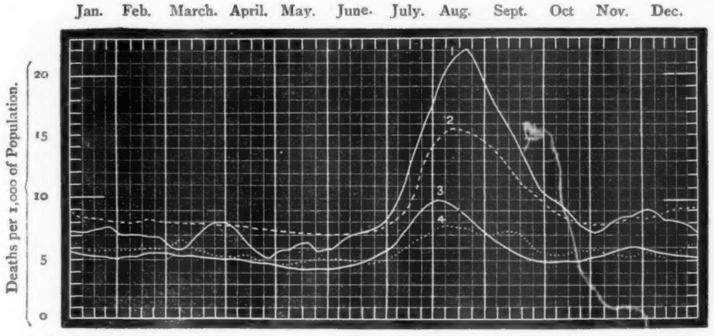


FIG. 1.—Showing the Weekly Deaths among Infants under one year of age on the Annual Mortality per 1,000 of the whole population. For Leicester, Curve 1; Liverpool, 2; London, 3; and Bristol, 4.

It is, however, to the summer death-rate that the greatest nterest attaches, since it is during the hottest weeks of the year that the differences in the rates of mortality of the different British towns stand most prominently out. During the period of high temperature in summer, *every one of the large towns of England shows an excess of deaths above the average*, with the single exception of Bristol, at which place, while there occurs an increased mortality at this season, it only comes near to, but never quite reaches, the average. As regards the time of absolute maximum, it occurs in London in the end of July, but at other places more generally about the beginning and middle of August. Taking any two consecutive weeks which indicate the highest mortality, the excess per cent. above the average is for Wolverhampton, 6; Manchester, 8; Portsmouth, 12; London, 14; Hull, 20; and Leicester, 47. The excess above the average at Leicester being thus eight times greater than that of Wolverhampton.

In Scotland no town exceeds its average during the hottest weeks of the year, but on the contrary the death-rate everywhere is under the average, and in most cases very considerably so. At Aberdeen the rate below the average is 18 per cent. during each of the months, July, August, and September; and at Dublin the annual minimum occurs in July, when the death-rate falls 25 per cent. below the average during the second and third weeks of that month. Though none of the Scottish towns exceed the average at this season, yet Glasgow and Dundee show a decidedly increased mortality, their curves though rising towards never quite reach the average.

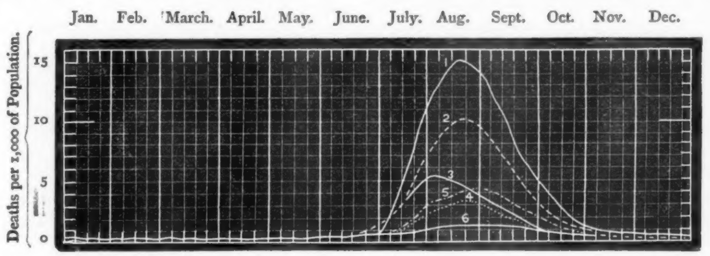


FIG. 2.—Showing the Weekly Deaths from Diarrhœa on the Annual Mortality per 1,000 of the whole population. For Leicester, Curve 1; Liverpool, 2; London, 3; Bristol, 4; Portsmouth, 5; and Edinburgh, 6.

In a paper on the mortality of London by Dr. Arthur Mitchell and myself, it has been shown that if the deaths of children under one year of age be deducted from the total mortality, the summer excess disappears from the curve; and it is further showni that, if deaths from diarrhœa be deducted from the whole mortality, the summer excess disappears equally as in the former case. Now, these results hold good for every one of the large towns for which the required data have been published. It follows, therefore, that curves of the death-rate for infants and diarrhœa have a peculiar interest in discussions of this nature. Fig. 1 gives for Bristol, London, Liverpool, and Leicester curves representing the average weekly mortality among children under one year of age, calculated on the annual mortality of 1,000 of the whole population, the averages being dealt with after Mr. Bloxam's method, according to which each average is calculated so as to include that of the week immediately preceding and that of the week immediately following.

Of all the large towns of England, Bristol has the least summer excess of infant mortality, the highest average of any week being only at the rate of eight calculated on an annual mortality of 1,000 of the whole population. In London, the rate rises to ten in the end of July and beginning of August; and in Liverpool it rises to sixteen, a rate which is also reached by the deaths in Leeds, Hull, and Sheffield, and closely approached by a number

* The substance of this paper was read at the general meeting of the Scottish Meteorological Society, held at Edinburgh on July 13, 1875.

of the other English towns. At Leicester, however, it shoots up to twenty-two, and twenty-four on the second and third weeks of August. As regards the whole year, the lowest averages of infant mortality are—Portsmouth, 4·9; London, 5·7; and Bristol, 5·9: and the highest, Leeds, 8·4; Liverpool, 9·1; and Leicester, 9·4. The season of minimum infant mortality is everywhere during the spring months in the sixteen large towns of England. The smallest spring mortality occurs at Portsmouth, the smallest summer mortality at Bristol, the largest summer mortality at Leicester, and the largest mortality during the other nine months of the year at Liverpool.

Fig. 2 shows the distribution of the mortality from diarrhœa through the weeks of the year, in six large towns, the curves being constructed similarly to, and on the same scale as, those of Fig. 1.

The differences in the rates of mortality from diarrhœa indicated by these curves, which are strictly comparable *inter se*, are very great, and a comparison of the two extremes, Leicester and Edinburgh, is startling; the figures showing that for every one who dies from diarrhœa in Edinburgh during the summer months, eight die in Leicester from the same disease in proportion to the population.

From the beginning of November to the summer solstice, the mortality from diarrhœa is everywhere small, being double, however, in Liverpool and Manchester as compared with London and Portsmouth. It will be observed from Fig. 2 that the curves begin to open out and diverge from each other in the end of June. The curve for Edinburgh on no week reaches the annual rate of 2 per 1,000 of the population. The highest for any week are—Bristol, 3·6; Portsmouth, 3·9; London, 5·5; Liverpool, 10·5; and Leicester, 15·8, these two last places again standing higher than any other of the towns.

The following is a list of all the large towns of Great Britain, arranged in the order of the greater or less prevalence of fatal cases of diarrhœa, during July, August, and September, the figures being the average weekly death-rate for the thirteen weeks, calculated on the annual mortality per 1,000 of the population:—*In England:* Leicester, 9·56; Salford, 7·15; Leeds, 7·02; Manchester, 7·00; Liverpool, 6·28; Sheffield, 6·20; Birmingham, 5·78; Hull, 5·56; Nottingham, 5·36; Norwich, 5·02; Newcastle, 4·61; Bradford, 4·42; Wolverhampton, 4·03; Sunderland, 3·89; London, 3·45; Portsmouth, 2·94; and Bristol, 2·38; and *in Scotland:* Dundee, 2·14; Glasgow, 1·90; Greenock, 1·75; Paisley, 1·71; Leith, 1·45; Edinburgh, 1·23; Perth, 1·08; and Aberdeen, 0·96.

From these results it will be seen that the influence of climate is unmistakable. The summer temperature of the Scottish large towns is several degrees lower than that of the English towns, and we see that every one of the Scottish towns has a mortality from diarrhœa lower than the lowest mortality of any one of the English towns. Of all the large towns of Great Britain the lowest death-rate from diarrhœa is that of Aberdeen, which is at the same time characterised by the lowest summer temperature. Further, the diarrhœa mortality of each town is found from year to year to rise proportionally with the increase of temperature, *but the rate of increase differs very greatly in different towns.* This points to other causes than mere weather, or the relative temperature and humidity of the place, as determining the absolute mortality. Thus the summer temperature of Dundee and Perth is nearly the same, and that of Glasgow and Edinburgh is also nearly alike, the excess being rather in favour of Perth and Edinburgh; and yet the diarrhœa mortality of these two towns is respectively less than that of Dundee and Glasgow. It may therefore be assumed that there is something in the topographical, social, or sanitary conditions of Dundee and Glasgow, which intensifies the evil effects of hot weather on the health of the people, so as to swell, for instance, the death-rate from diarrhœa at Dundee to double that of Perth. At Leicester the summer temperature does not exceed that of Bristol; but while the summer death-rate from diarrhœa at Bristol is 2·38, at Leicester it is 9·56; in other words, it may be assumed that there are local peculiarities affecting the population of Leicester, the effect of which is to quadruple the death-rate from diarrhœa in that town as compared with Bristol. It is to these local conditions we must look for an explanation of the great differences in the death-rate of the different towns. The highest average death-rate per annum for the period under discussion is Liverpool 30·6, Glasgow 30·5, Manchester 30·2, Greenock 29·3, and Paisley 29·0; and the lowest is Portsmouth 20·6, London 23·2, and Aberdeen 23·3. Thus, for every two who die at Portsmouth, three die at Liverpool, Glasgow, and Manchester.

These facts suggest large inquiries which call for instant and serious attention. As one of the first steps of the inquiry, it is most desirable to know exactly from a weekly registration of the facts, whether the infant mortality is equally distributed among all infants, however nursed, or whether it may not rather be distributed among them in very unequal proportion, according to the manner in which they are fed. Those, for instance nursed at the breast may be much less liable to succumb to diarrhœa in summer than those fed on cow's milk or those fed on slops. The unusually low temperature of December last very largely increased the death-rate everywhere in the British Islands, particularly from diseases of the respiratory organs and from many diseases connected with the nervous system and the skin. The gross number of deaths registered in the different large towns showed that the excess of deaths thereby caused was very unequally distributed over the country. If there had been a more complete system of registration, for all the large towns, it might have been possible, reasoning from the specific diseases which proved to be unusually fatal at each place, to lay the finger on those local conditions, inimical to health, to which the high rate of mortality in each case was due. During the cold months of the year—December, January, and February—the mortality among females is very considerably in excess of that among males in London; for while during these thirteen weeks the average death-rate among males rises 7·8 per cent. above the weekly average of the year, the death-rate among females rises to 11·2 per cent. above the average. Since the facts of mortality for sex are only registered for all causes and all ages, it is impossible to say from the present system of registration how much of the excess of mortality among females in winter is due to sex, and how much to occupation, or even to fashion.

A comparison of the meteorological with the mortality records shows in an impressive manner the influence of particular types of weather in largely increasing or diminishing the number of deaths from particular complaints. Thus, periods of unusual cold combined with dampness in the end of autumn, cold with drought in spring, cold in winter, or heat in summer, are accompanied with a proportionally increased mortality from scarlet fever, whooping-cough (if these diseases be epidemic at the time), bronchial affections, and bowel complaints respectively. Again, as regards diarrhœa, for example, there appear to be certain critical temperatures, such as 55°, 60°, 63°, and 65°, at which as they are reached, the mortality rises successively to greatly accelerated rates. To work out the problem of the relation of the weather and mortality of our large towns, it is indispensable for the comparison of the different towns with each other, that the system of observation be uniform at all places, particularly as regards the hours and modes of observing the temperature, humidity, and movements of the air, and the rainfall; and it is further indispensable that several meteorological stations be established in each of the large towns.

ALEXANDER BUCHAN

SCIENTIFIC SERIALS

Mental Science Journals, January, April, July.—The January number opens with an article by Samuel Wilks, M.D., "The Study of the Human Mind from a Physiological View." Dr. Wilks finds no more difficulty as regards the relation of the mind and brain than in "the association of other functions with their respective organs." The main purpose of the writer seems to be to show that men are very much of automata. In this he thinks he has followed Dr. Huxley, who however, if he meant anything, meant that men are altogether automata. The illustrations of the automatism of doctors must be alarming to the nervous and ailing. Example: "Up to the present time I have never seen a single case of leucocythæmia of the lympathic glands, or the spleen, or simple idiopathic anæmia, without the patient's having been saturated by iodine, quinine, and iron; but no case is yet recorded of these remedies having done the slightest good."—David Nicholson, M.B., continues his "Morbid Psychology of Criminals," and shows his vigorous common sense in refusing to see that suicide is always an insane act, or that there is any "madness in an idle-minded fellow preferring to live 'like a gentleman' by helping himself directly from moneyed pockets, instead of sweating his life out with a pick

and shovel at fourteen shillings a week."—This number contains an interesting paper on the Hallucinations of Mahomet and others, by W. W. Ireland, M.D.—In the April number we find the Morisonian lectures on Insanity for 1873, this time written entirely by Dr. Clouston; the Morbid Psychology of Criminals continues; an article on the Family Care of the Insane in Scotland, by Prof. Friedrich Jolly, of Strasburg, is valuable, inasmuch as it helps us "to see ourselves as others see us," and pleasing, as this time we may look and be not ashamed. "This visit," says Prof. Jolly, "and the information furnished by these gentlemen, as well as a more careful study of the Scottish Reports and their appendices, convinced me that it is no 'Gheel in the North' with which we have to do, but an organisation which rests on a quite different and much sounder basis."—George Shearer, M.D., communicates notes to show that "Diseases of the general nervous system are by no means infrequent amongst the Chinese, but cases of alienation of mind are comparatively few."—Mr. E. Thompson continues and concludes his paper on the Physiology of General Paralysis of the Insane and of Epilepsy. The worst things in the paper are a few unseemly remarks directed against Dr. Hughlings Jackson.—The July number opens with a Chapter on some Organic Laws of Personal and Ancestral Memory, by J. Laycock, M.D.—The Morisonian lectures on Insanity are continued from the previous number.—David Nicholson M.B., furnishes his excellent articles on the Morbid Psychology of Criminals, which we have always read with much pleasure.—S. Messenger, F.R.C.S., writes under the title, "Moral Responsibility," to show that we all are what we are because, given our parents and our circumstances, we could not have been otherwise. The moral of "this theory of no-moral of responsibility" is very good, "we should be more generally charitable in our judgments, more universal in our forbearance." It is a pity that the men who are continually claiming to be the only scientific men cannot reach such simple conclusions without outraging language and common sense in order to show, by the way, that they are not metaphysicians. Mr. Messenger describes the manufacture of thought as similar to the manufacture of gastric juice—the action of the brain is like "that of the stomach, whose peptic glands secrete the gastric juice from the circulating blood, but need the stimulus of food to excite the process." It would be a great advantage to the scientific men of this stamp if they would try "the means of observation which metaphysicians employ," or any other that might help them to see that intelligence is not a juice.

In the *Scottish Naturalist* for April and July 1875, the difficult subject of the relationship between the mental development of man and of the lower animals occupies a rather prominent place, in a series of articles by Dr. Lauder Lindsay, on "Illustrations of Animal Reason," and one by the Rev. J. Wardrop, on "Animal Psychosis."—The former writer also contributes a paper on "The Auriferous Quartzites of Scotland," in which he brings forward evidence in support of the view long since published by him of the auriferous character of the whole Lower Silurian area of Scotland; the rocks being identical stratigraphically with those of the gold-fields of the province of Otago, in New Zealand.—There are several other good geological papers, especially one by Mr. R. Walker, "On Clays containing *Ophiolepis gracilis*, &c., near St. Andrew's."—The zoological and botanical papers are mostly descriptive, and we have continuations of the "Lepidoptera of Scotland," by Dr. Buchanan White, and the "Coleoptera of Scotland," by Dr. D. Sharp.

The numbers of the *Journal of Botany* from March to July contain many articles of interest; and nearly every number is now illustrated by at least one original drawing. Those in the numbers now under notice are the fruit of the Bitter Cola, an undescribed species of Clusiaceæ from Wesetrn Tropical Africa, nearly allied to *Garcinia*, several species or new or rare Hymenomycelous Fungi (coloured), *Deidamia Thompsoniana*, a remarkable species of Passifloraceæ, and *Carex ornithopoda*, a newly discovered British species. Besides a number of abstracts and short notes, the following are the more important original articles in these numbers:—Descriptions of a number of new and unpublished species by Dr. Masters, Dr. Trimen, Mr. J. G. Baker, Dr. Hance, and others. In Cryptogamy, Mr. Worthington Smith describes a number of new species of fungi; the Rev. J. M. Crombie the additions to the British lichen flora since his last notice; and Mr. J. G. Baker several new ferns. One of the best papers in these numbers is by Mr. A. H. Church, with an account of some recent investigations in phyto-chemistry at the laboratory at Cirencester. An analysis of the dried substance of a fungus (*Geoglossum difforme*), and of a lichen (*Collema furvum*), showed the former to contain 19 and the latter as much as 28 per cent. of albuminoids; while the former contains the very large proportion of 13·87, and the latter 6·57 per cent. of ash. Cotton, generally considered to be almost pure cellulose, was analysed with the following result:—

Water	7·56 per cent.
Oil and fat	0·51 ,,
Albuminoids	0·50 ,,
Gummy matters	0·17 ,,
Ash	0·11 ,,
Cellulose	91·15 ,,
	100·00

The composition of the pollen of *Cupressus fragrans* was determined as under:—

Carbohydrates and undetermined	85 76 per cent.
Oil and fat	1·87 ,,
Albuminoids	8 67 ,,
Ash	3·70 ,,
	100·00

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, June 15.—This number contains a paper by Herr Hellmann, of Berlin, on the extension of a short series of observations on temperature by means of the long series of a neighbouring station. It was one of Dove's result's that series of mean temperatures of two neighbouring places derived from a different number of years might be reduced so as to extend over equal periods. His hypothesis has proved a fruitful one. The object of Herr Hellmann was to confirm its value, and this he did by taking mean temperatures already obtained by observation for long and equal periods at two neighbouring places; then assuming that, say for the last five years, no observations had been made at one of them, and calculating from those of the other the required means for the whole period. The difference between the real values and those calculated expresses about the amount of error incurred, which is surprisingly small. Thus, out of eighty-four monthly means for seven pairs of similarly situated stations, only four differences exceeded one-tenth of a degree. But when a hill station is compared with a valley station the agreement is not so good, and it appears that with increase of height the climate becomes more uniform; between an inland and a coast station the difference is still greater, but rarely exceeds half a degree. We may conclude that observations made at a place situated on a plain may safely be employed for the extension of a shorter series of observations made at another place at no great distance, similarly situated, and that the error will be greater when stations different in position are compared.

July 1.—This number contains a review of Mr. Symons's publications on British Rainfall, by M. Raulin, of Bordeaux, and, among the "Kleinere Mittheilungen," a paper on the production of centres of cold in winter.

SOCIETIES AND ACADEMIES

London

Royal Horticultural Society, July 7.—General Meeting.—Hon. and Rev. J. T. Boscawen in the chair.—The Rev. M. J. Berkeley briefly alluded to Mr. Worthington Smith's paper before the Scientific Committee.

July 21.—Scientific Committee.—M. T. Masters, M.D., F.R.S., in the chair—Mr. Bennett exhibited a fine specimen of a fasciated cucumber stem bearing two cucumbers.—Mr. W. G. Smith made a further communication on the resting spores of the potato fungus.—A letter was read from Mr. C. E. Broome, including a sketch of *Diplodia*-like bodies met with in the mycelial filaments.—Mr. Renny made a communication on the same subject, and exhibited a species of *Pythium* (*Saprolegnieæ*), which, without care, might be mistaken for the state of *Peronospora* described by Mr. Smith.—A letter was read from Lady Mathison, accompanying specimens of various larvæ which proved very destructive to the otherwise thriving plantation of Falkland Island Tussock grass (*Dactylis cæspitosa*) in the island of Lewis.—Mr. Alfred Bennett called attention to the rapid growth of the flower-stalk of *Vallisneria spiralis*, which he had observed to grow as much as 12 inches in twenty-four hours.—A letter, communicated by Dr. Hooker, F.R.S., was read from Dr. Imray, of

Dominica, accompanied by an excellent series of specimens of the minute moth (*Cemiostoma coffeella*) which injures the leaves of the coffee plant in Dominica.—Dr. Masters exhibited a flower of a hybrid *Tacsonia* in which the anthers were replaced by petals, while from the apex of the tube formed by the filaments, a second corona of blue threads proceeded. Dr. Masters also exhibited a flower of a *Cattleya*, in which there were three equal sepals and four petals all lip-like. From the arrangement of the parts Dr. Masters concluded that there was in this specimen a passage from the whorled to the spiral arrangement.

General Meeting.—Hon. and Rev. J. T. Boscawen in the chair.—The Rev. M. J. Berkeley commented upon the objects exhibited and also upon Mr. W. G. Smith's further observations upon the resting spores of the potato-disease fungus.—Prof. Thiselton Dyer made some observations upon a fine pan of Droseras from the New Forest, exhibited by the Chairman.—Dr. Masters commented on the splendid pitchers of *Nepenthes* sent by Mr. D. Thomson, gardener to the Duke of Buccleugh at Drumlanrig.

Quekett Microscopical Club, July 23.—Annual Meeting.—Dr. Matthews, president, in the chair.—The report showed that the club had completed the tenth year of its existence and that it continued to make most satisfactory progress; the meetings had been well attended, excellent papers had been read and useful work accomplished, whilst the library and cabinet were in good order, and the field excursions had been very successful. The treasurer's statement showed that the year's income from all sources amounted to 291*l.* 13*s.* 11*d.*, and that there was a balance in hand of 73*l.* 9*s.* 9*d.* Votes of thanks to the president and officers were duly passed, as was also a special vote of thanks to the Council of University College for their continued kindness in allowing the meetings to be held in the library of that building.—The annual address was delivered by the President, and upon its conclusion a ballot took place for the election of officers and committee for the ensuing year. Dr. J. Matthews was re-elected President. Messrs. J. Crisp, R. T. Lewis, B T. Lowne, and T. C. White, Vice-Presidents. As Hon. Sec., Mr. Ingpen; as Treasurer, Mr. Gay. Hon. Sec. for Foreign Correspondence, Dr. M. C. Cooke. And to fill the four vacancies on Committee, Messrs. M. H. Johnson, F. Oxley, T. Rogers, and Joseph A. Smith.

BERLIN

German Chemical Society, June 28.—A. W. Hofmann, president, in the chair.—Messrs. von Dechen and Wichelhaus have studied the action of nitrobenzole on aniline. They obtain an amorphous violet colouring substance to which they give the formula $(C_6H_4)_3''N_2$; explaining its formation by the equations—

$$1.\quad \underset{\text{Nitrobenzole.}}{C_6H_5(NO_2)} + \underset{\text{Aniline.}}{2C_6H_5NH_2} = H_2O + \underset{\text{Violaniline.}}{(C_6H_5)_3N_3}$$

$$2.\quad (C_6H_5)_3N_3 = NH_3 + \underset{\text{(new substance).}}{(C_6H_4)_3''N_2}$$

Messrs. Oppenheim and Pfaff have continued their researches on oxyuvitinic acid, $C_6H_2\left\{\begin{array}{l}OH\\CH_3\\(CO_2H)_2\end{array}\right.$. They have prepared the methylic ether and the first anhydride of this acid, which they have found to be produced not only by the action of chloroform but also by that of chloral, of trichloroacetic ether, and of the chloride of carbon $C\,Cl_4$ on the sodium-compound of acetic ether. They have prepared 150 grammes of pure cresol from this acid and by transforming this cresol into cresotinic acid, methylic and ethylic ethers, methyloxybenzoic, ethyloxybenzoic, and oxybenzoic acids, and studying the properties of these bodies they have determined the cresol obtained to be *metacresol*. This leads them to allege the following position to the lateral groups of oxyuvitinic acid—

$$CH_3 : OH : CO_2H : CO_2H = 1 : 3 : 4 : 6.$$

The same chemists have found anisic acid to have the melting-point 184°·2 and methyloxybenzoic acid 106-107°, the melting-points formerly given being 10° too low.—P. Griess has transformed diazocyanobenzol into cyanophenol, by heating its sulphate with water—

$$C_6H_3CN\,N_2 + H_2O = C_6H_4CN\,OH.$$

Hydrochloric acid splits it into ammonia and meta-oxybenzoic acid. The cyananiline necessary for preparing the diazo compound had been prepared by heating uramidobenzoic acid with phosphoric anhydride—

$$C_6H_4\begin{array}{l}-NH-CO-NH_2\\-COOH\end{array} = C_6H_4\begin{array}{l}CN\\NH_2\end{array} + CO_2 + H_2O.$$

—A. Ladenburg has repeated Mr. Fittica's experiments without obtaining a trace of his presumed and inexplicable isomers of Nitrobenzoic acid.—O. Witt, by treating diphenylamine with nitrous ether has transformed it into yellow brilliant crystals of diphenylnitrosamine $N\begin{array}{l}/C_6H_5\\-NO\\\backslash C_6H_5\end{array}$.—A Pinner has transformed $C_3H_2Cl_2$ into a nitrochloroallylene, which, with tin and hydrochloric acid yields $C_3H_4Cl_3NH_2$ trichloropropylamine. Sodium acts on $C_3H_2Cl_2$ in a peculiar way. It forms with it a solid compound decomposed by water into chloride of sodium and C_3H_2 a gas forming the bromide $C_3H_2Br_2$.—A. W. Hofmann has distilled the compound ammonium $(CH_3)_3NC_2H_3OH$, hoping to obtain vinylic alcohol; he obtained, however, trimethylamine, water, and acetylene.

PARIS

Academy of Natural Sciences, July 26.—M. Fremy in the chair. The following papers were read:—Researches on the theory of aberration, and considerations on the influence of the proper motion of the solar system in the phenomenon of aberration, by M. Yvon Villarceau.—On the latitude of Abbadia near Hendaye (Basses Pyrenees), by M. A. D'Abbadie.—On the distribution of magnetism in compound bundles of very thin bars of finite length, by M. J. Jamin.—Note by M. Chevreul on the Compte Rendu of the meeting of July 19.—Complementary notice on the contemporaneous formation of minerals by the thermal springs of Bourbonne-les-Bains (Haute Marne); production of phosgenite, by M. Daubrée.—Researches on the phenomena produced by electric currents of high tension, and their analogy with natural phenomena, by M. G. Planté.—Action of electrolytic oxygen on glycerine, by M. Ad. Renard. The author finds as the result of this action formic and acetic acids and the first glyceric aldehyde.—Study of the pyrites employed in France for the manufacture of sulphuric acid, by MM. A. Girard and H. Morin.—On the toxic properties of the fermentation alcohols, by MM. Dujardin-Beaumetz and Audigé.—On amyloxanthate of potassium (for the destruction of Phylloxera), by MM. Zoeller and Grete.—On the thermal phenomenon accompanying inversion, by M. G. Fleury. The author concludes that the inversion of sugar by acids is an exothermal phenomenon.—Note on a substance serving to adulterate guanos, by M. F. Jean.—New researches on germination, by M. P. P. Dehérain.—Experiments showing that the mammæ removed from young female guinea pigs are not reproduced, by M. J. M. Philipeaux.

BOOKS AND PAMPHLETS RECEIVED

BRITISH.—British Wild Flowers. Parts 14 and 15: J. E. Sowerby (Van Voorst).—Sound. New Edition: J Tyndall, D.C.L., LL.D., F.R.S. (Longmans).—Six Lectures on Light, delivered in America, 1872–73. New Edition: J. Tyndall, D.C.L., LL.D., F.R.S. (Longmans).—Geometrical Contributoins to the *Educational Times*: T. Archer Hirst, F.R.S. (Hodgson and Son).—Report of the Inspectors of Irish Fisheries for 1874 (Dublin, Thom).—Insectivorous Plants: Charles Darwin, M.A., F.R.S. (John Murray).

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